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XV. *Simplified Inductance Calculations, with Special Reference to Thick Coils.* By PHILIP R. COURSEY, *B.Sc., A.M.I.E.E.*

RECEIVED JANUARY 15, 1919.

1. The predetermination of the inductance of coils by direct arithmetical calculation is a subject to which a great deal of attention has been devoted. The results are seen in the many published Papers giving formulæ for this purpose. Most of these formulæ have been obtained by rigid mathematical deduction, but some are empirical. Many are very complicated, and unsuited for easy computation.

Several writers have attempted the simplification of some of these formulæ to render them more suitable for everyday use. Often this has resulted in a limitation of their range of applicability to certain selected cases, and in some instances confusion has arisen through imperfect statement of the limits between which the particular formula may be employed.

2. *Notation.*

Let—

L=Inductance of coil in centimetres.
D=Mean diameter of coil (cm.)$=2a$.
a=Mean radius of coil.
l=Axial length (cm.).
d=Radial depth of windings (cm.).
N=Total number of actual turns of wire.
$n=N/l$=Number of (actual) turns of wire per centimetre of length.
n'=Number of (imaginary) turns of square wire that can be fitted into coil section$=l/d$.
k=Correction factor for single layer windings.
δk=Correction for coil thickness.
k'=Correction factor for thick coils.
$\Delta_1 L$=Rosa's Correction for coil thickness.
m, A, B, &c.=Other factors used in various formulæ.

3. *Single Layer Coils.*

The calculation of the inductance of single-layer coils has, perhaps, received most attention in the direction of simplification on account of the practical requirements in the realm of wireless and other high-frequency apparatus. Several

abacs, charts and curves have been published with this end in view.

When dealing with single layer coils one of the most useful, and, at the same time, most accurate and universal of the available formulæ is that of Nagaoka.* The main part of this formula, viz. :—

$$L=\pi^2D^2n^2lk \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is very easily dealt with, but the expressions for calculating the factor k for the various cases of long or short coils are very cumbersome and tedious to use. Extensive tables of this factor have, however, been published in the "Bulletin" of the Bureau of Standards, and elsewhere, giving the values of k in terms of the ratio D/l=diameter÷length of the coil.

The use of this formula in practice may be simplified by plotting curves of the values of k and reading the value required from these. This method has other special advantages when the design of a coil to have any predetermined value of inductance is under consideration, as has been previously pointed out by the author.† The general form of this function k plotted against the ratio l/D is given in Fig. 1.

The results given by this formula are, strictly speaking, liable to a correction for the insulation or spacing of the turns of wire, but in practice this correction is a small one (usually less than 1 per cent.), and may be neglected—at least to the approximation required for most practical work—for which curves are applicable.

This formula may be expressed in a number of different ways if so desired. Such modifications have given rise to a number of abacs and charts to simplify its use. In general, however, they amount to the same result as that given by the curves above, while their range is usually much more limited.

As an example we may mention Eccles's abac given in his "Handbook of Wireless Telegraphy." This is based on Russell's formula, and is written in the form

$$L=ma^3n^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Values of m are obtained from the abac in terms of the ratio l/D (Fig. 2).

* Nagoaka, "Journal" College of Science, Tokyo, XXVII., p. 18 (1909); also "Bulletin" of Bureau of Standards, Washington, VIII., p. 119 (1912).

† P. R. Coursey, "Electrician," LXXV., p. 841 (September, 1915).

If we equate this expression to the one used for the curves of Fig. 1, we have

$$m(D/2)^3n^2=\pi^2D^2n^2lk,$$

or,
$$m=8\pi^2\frac{lk}{D}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

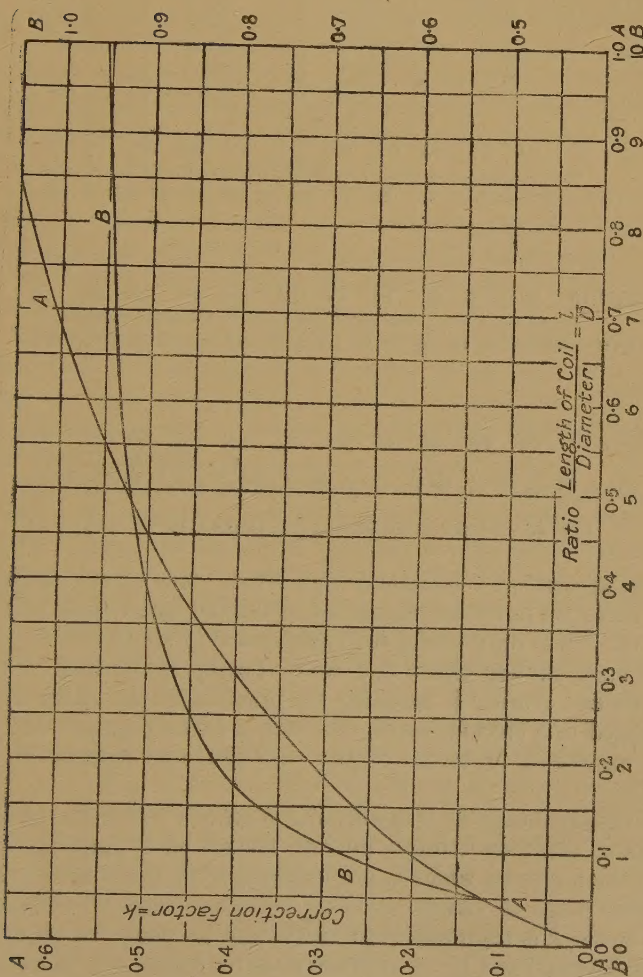


Fig. 1.—Curves of Correction Factor for Single layer Coils.

We may therefore compare the results obtained by the two methods by working out values of $8\pi^2[lk/D]$.

M 2

Curves of lk/D were given by the author in the "Electrician"* for the purposes of the design of coils. Using these curves, we obtain the following table :—

l/D.	lk/D, from curve.	$8\pi^2 lk/D$.	m from abac.	Difference, per cent.
0·2	0·063	4·96	5·00	+0·8
0·65	0·384	30·3	30·2	−0·33
1·5	1·15	90·8	91·0	+0·22
2·4	2·04	161·0	160·0	−0·62
4·2	3·80	300·0	300·0	0·0
5·2	4·78	384·0	379·0	−1·3

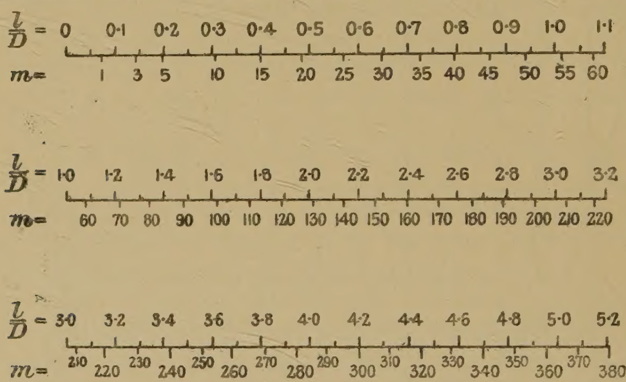


FIG. 2.—ABAC FOR SINGLE-LAYER COILS (DR. ECCLES).

The agreement is evidently very close. The useful range of the abac is, however, much more limited than that of the curves. It is only useful for comparatively short coils.

A very similar chart due to S. Lowey, published in the "Wireless World,"† amounts to practically the same abac arranged in a circular form, but with a slightly greater range, and expressed in terms of the coil diameter instead of its radius.

A disadvantage of transforming Nagaoka's formula in this manner, viz. : splitting it up into factors of D^3, and lk/D, is that the factor m, which is proportional to lk/D, covers a much greater range of values—varying between 0 and ∞ —whereas the factor k is asymptotic to unity, and its value for all cases lies between 0 and 1. A curve or abac for k does not, therefore, require to be so extended.

* P. R. Coursey, "Electrician," *loc. cit.*

† S. Lowey, "Wireless World," III., p. 664 (January, 1916).

4. *Thick Coils.*

When we come to consider the case of coils having a radial thickness that is not negligible compared with either the diameter or the length, these simplified formulæ become very inaccurate if used as they stand. Moreover the usual accurate expressions for these cases are not easy to work with in the forms usually given. These coils—"thick coils," as we may term them for distinctive purposes—have not perhaps quite such a wide sphere of use as the single layer coils, but nevertheless they are of importance in some branches of engineering, and are finding more extended use for high-frequency work than hitherto.

It is understood, of course, that these calculations always refer to "air-core" coils, or to be more general, to all coils having a core of permeability unity, as the inductance of iron-cored coils is too uncertain and variable a quantity to require predetermination in most cases.

The principle object of this Paper is to indicate how the simplified form of Nagaoka's formula, using the "k" curves, may be adapted to a general method of calculation applicable to all types of coils, whether thin or thick, short or long, of one turn or many.

In the single layer case considered above, the magnetic flux passing through the centre of the coil is linked with practically all the turns—the factor k correcting for the spreading of the flux at the ends of the coil. Evidently when more layers are added to the coil, the flux due to the inner layers does not link directly with the outer layers of wire, so that the effective inductance is less than it would be if the same number of turns were all concentrated upon a single outer layer. It should, therefore, be possible to allow for this reduction in inductance by introducing an appropriate reduction in the factor k. For this purpose a series of values of a factor δk have been calculated. This factor is to be subtracted from the proper value of k obtained from the curves, and the new value k' used with the standard formula in the usual manner.

Hence we have

$$L=\pi^2 D^2 n^2 l(k-\delta k)$$

$$=\pi^2 D^2 n^2 l k', \qquad \ldots \ldots \quad (4)$$

where $k'=(k-\delta k)$.

A series of values of this reduction factor δk is shown in Fig. 3.

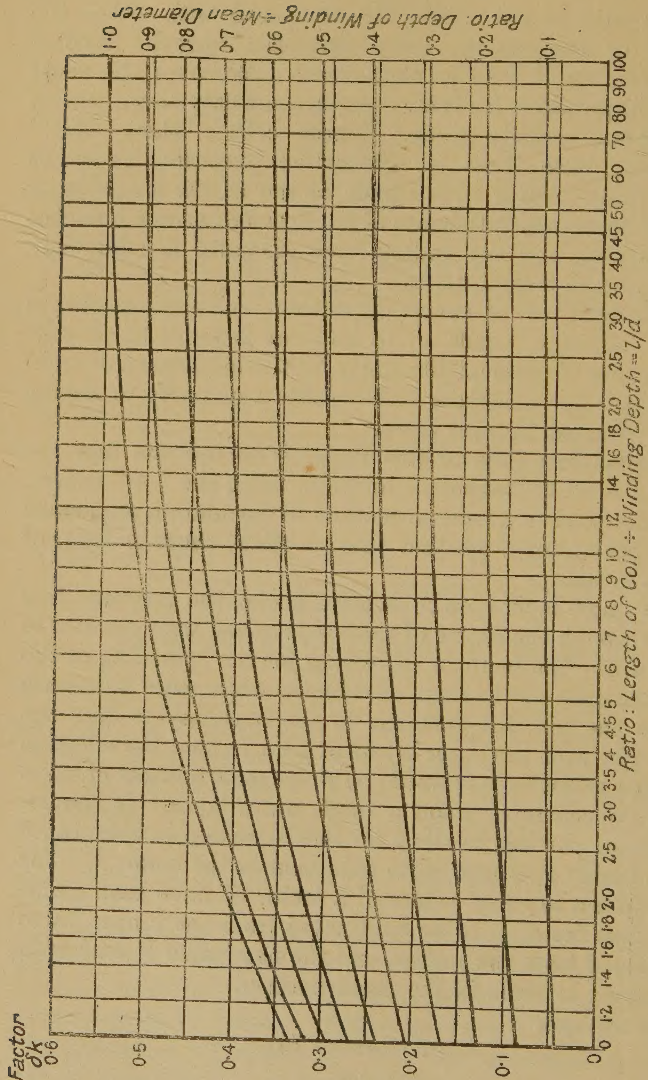


Fig. 3.—Curves of Factor δk for Thick Coils.

The method by which they were obtained may be indicated as follows :

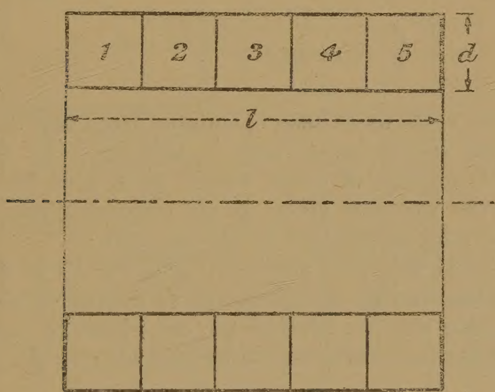
Using Rosa's formula for the inductance of a thick coil—one of the most accurate for this class of coils—we have at once an expression in the form required.

This is

$$L = L_s - \Delta_1 L + \Delta_2 L, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where L_s is the inductance calculated by any suitable formula for " current-sheets " or infinitely thin single-layer coils ; and $\Delta_1 L$ is a correction for the coil thickness given by

$$\Delta_1 L = 4\pi a n'[A_s + B_s]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$



$n' = 5$

FIG. 4.

A_s and B_s are factors tabluated by Rosa in the " Bulletin,"* $\Delta_2 L$ is a correction for the insulation of the wires, and, in most of the cases here dealt with, is quite negligible. The term n' in the expression (6) is the number of square conductors that can be fitted into the coil section, Fig. 4. Evidently $n' = l/d$.

The " current-sheet " inductance must be worked out for this number of turns n' to enable the correction $\Delta_1 L$ to be properly applied. The final result must be corrected to allow

* E. B. Rosa, " Bulletin " of Bureau of Standards, IV., p. 369 (1907); also VIII., p. 138 (1912).

for the actual number of turns N on the coil. Thus, using Nagaoka's formula for L_s, we have

$$L=L_s-\Delta_1 L$$

$$=\frac{N^2}{n'^2}\left\{\frac{\pi^2 D^2 n'^2}{l}k-2\pi D n'(A_s+B_s)\right\}$$

$$=\frac{\pi^2 D^2 N^2}{l}\left[k-\frac{2}{\pi}.\frac{l}{D}.\frac{1}{n'}(A_s+B_s)\right]$$

$$=\frac{\pi^2 D^2 N^2}{l}(k-\delta k),$$

where $$\delta k=\frac{2}{\pi}.\frac{l}{D}.\frac{1}{n'}(A_s+B_s)$$

$$=\frac{2}{\pi}.\frac{d}{D}.(A_s+B_s). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The values of δk have been calculated from this expression by using the values for A_s and B_s tabulated by Rosa. A considerable number of extra values have also been calculated from the formulæ given in his original Paper,* as the tables were found insufficient for this purpose.

Evidently not only is it possible to plot curves of δk, as has been indicated in Fig. 3, but a set of curves of the factor $k'=k-\delta k$ may also be plotted on one sheet to enable the appropriate factor for *any* coil to be read off at a glance. (*See* Fig. 5.)

These curves are plotted against the ratio l/D (=length ÷diameter), so that the k curve for a single layer coil can also be included. A wide range of values is obtained by the use of the logarithmic scale. In using the curves it should be noted that when the winding depth d is greater than the axial length l, the values of l and d should be interchanged in working out the inductance both in the formula and in finding the factor k'. The inductances of the short thick coil and the long thin one of Fig. 6 are practically the same, provided that the mean diameters are identical.

The results obtained from these curves have been compared with measurements on several coils, and have been found to be generally correct within a few per cent.

* E. B. Rosa, *loc. cit.*

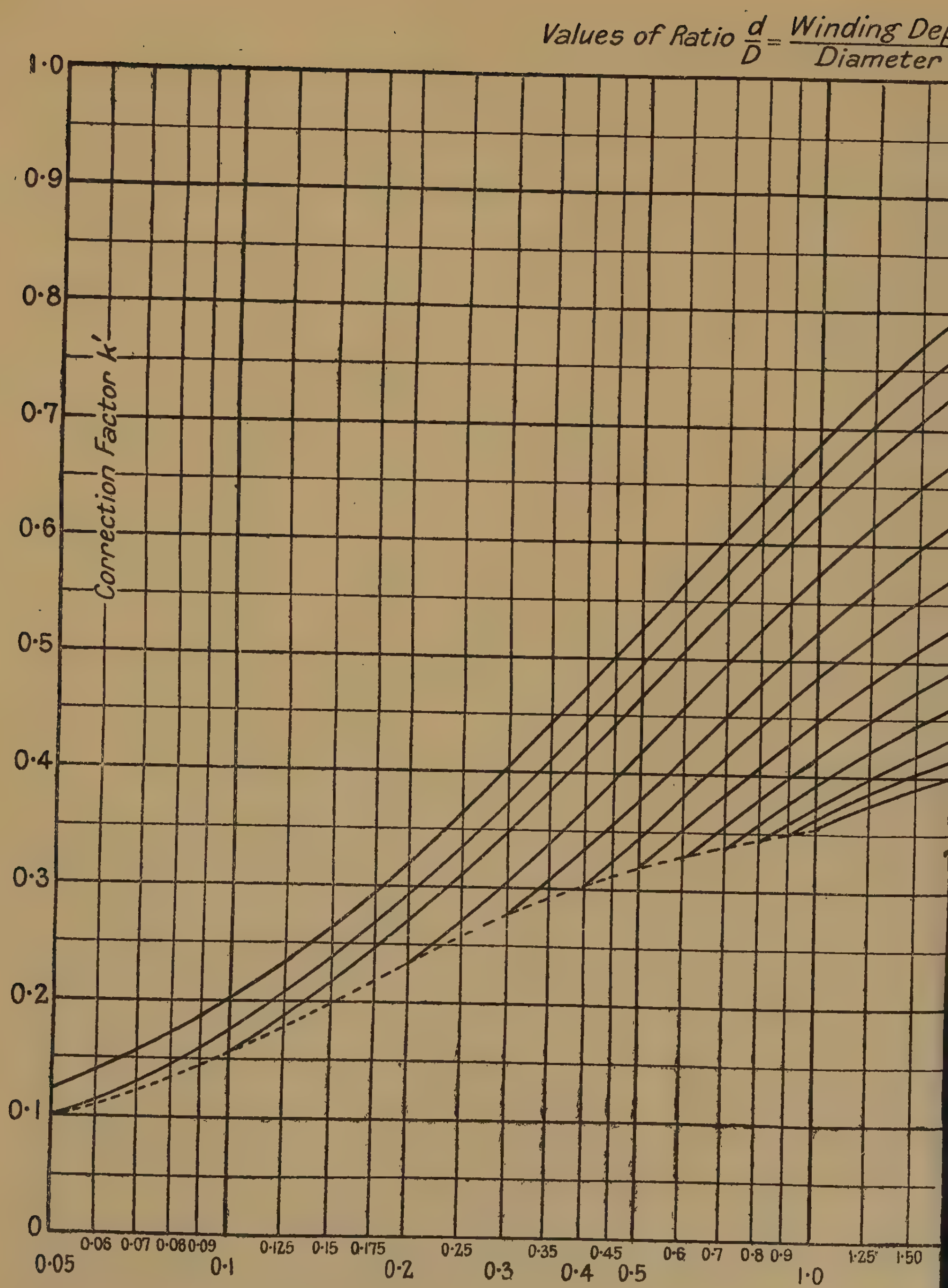
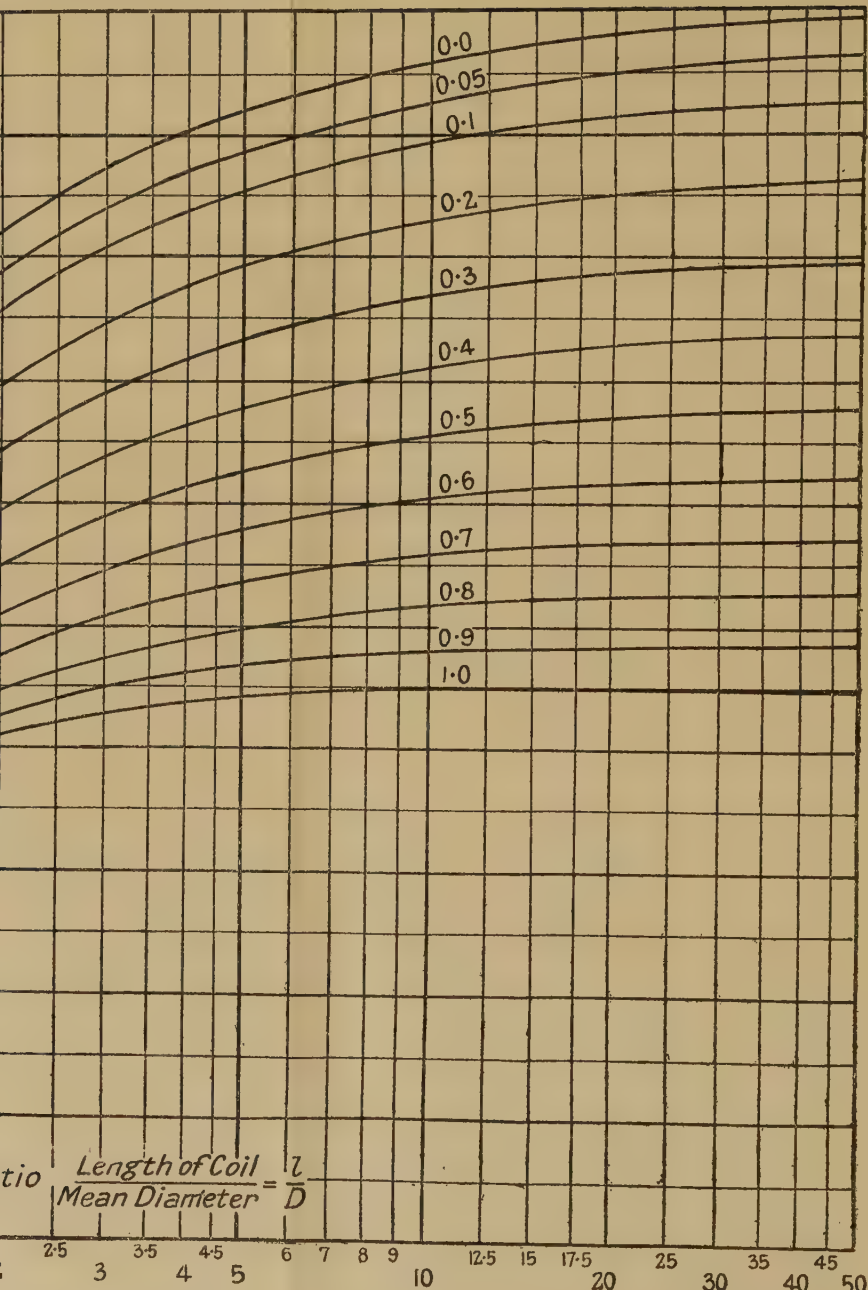


FIG. 5.—CURVES OF FACTOR k' FOR

marked on each curve



Coils (thick or thin).

[*To face* p. 162

These k' curves have an advantage in being asymptotic as compared with others, in which the formula has been broken up into factors of n^2, D^3, and m, as for the single layer case (Eccles's Abac, &c.), equation (2), or into factors of N^2, D, and $(A-B)$, as in L. A. Doggett's chart.* This last is shown in Fig. 7.

The values given by this chart compare very well with the k' curves in spite of the rather small scale of the chart as pub-

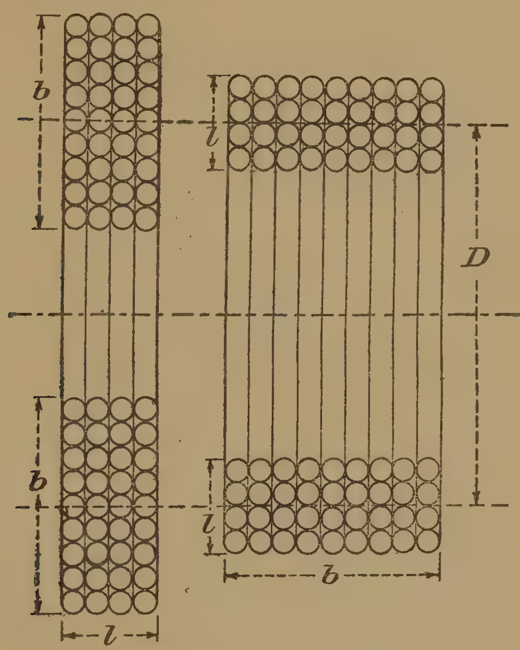


Fig. 6.—Equal Inductance Coils.

lished. Taking the example quoted on the chart (Fig. 7) the inductance using the k' curves is 2,190 cm., while that given by Doggett is 2,130 cm. This is rather an unfavourable case, and most results are nearer than this one. The k' curves in nearly all cases give a much more accurately readable value of the correcting factor.

The method of calculation adopted for the factor δk does not, in its present form, admit of its calculation for values of l/D

* L. A. Doggett, "Electrical World," January 31, 1914; "Electrica Review" (Lond.), LXXIV., p. 489 (1914).

$<d/D$, and it will be noted that in consequence the k' curves as given do not extend beyond this value. This point corresponds to the square section coil, length=depth. Shorter coils may be calculated to a first approximation by interchanging

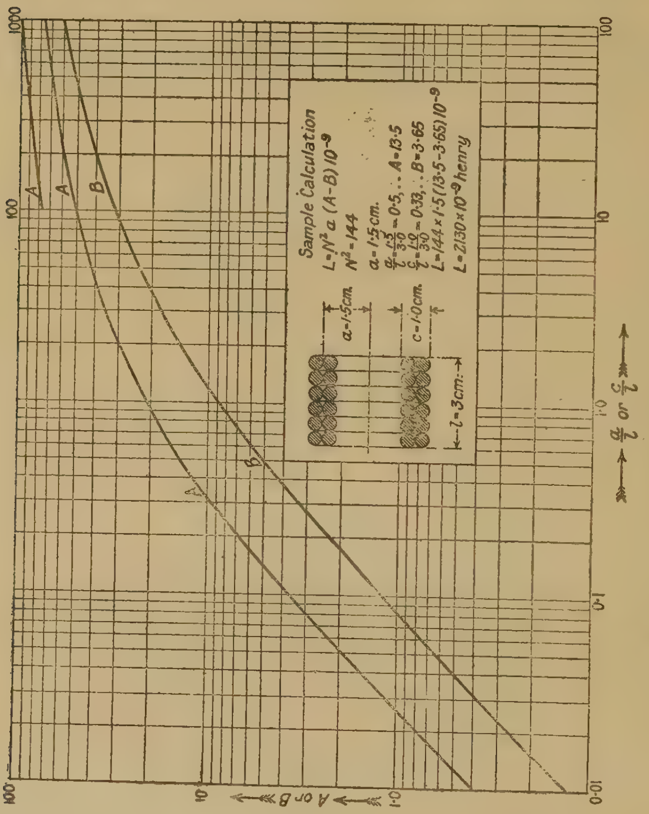


Fig. 7.—Chart for Thick Coils (L. Doggett).

l and d as indicated above. It is hoped in a later communication to deal with the extension of these curves to the smaller values of l by an alternative method of calculation.

6. *Single-turn Rings.*

Another use for these curves should also be noted—viz., their application to coils of very small section, down to the case of single turns of wire.

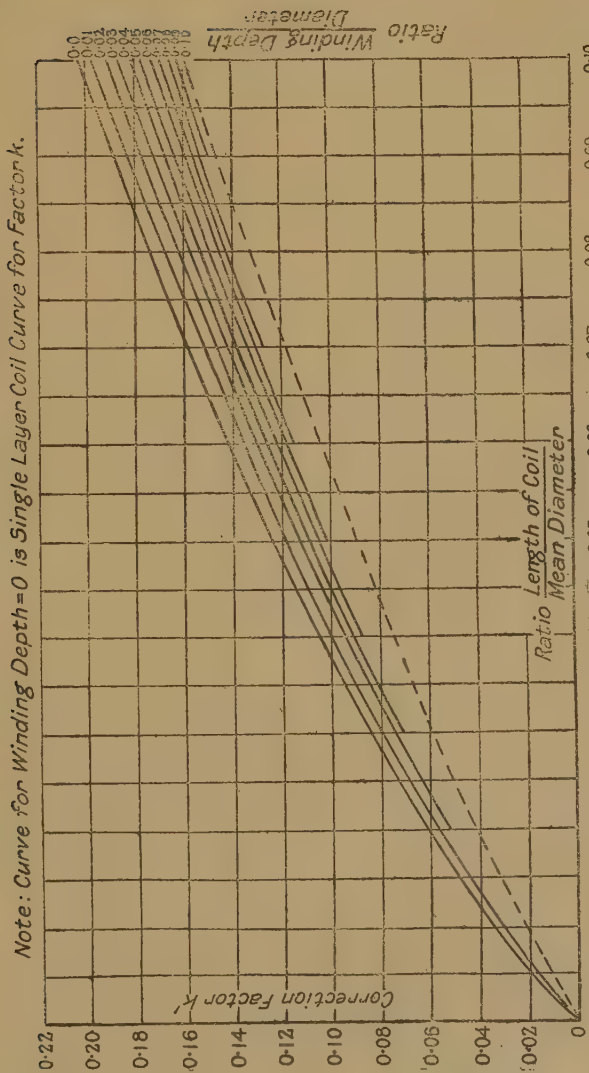


FIG. 8.—CURVES OF k' FOR COILS OF VERY SMALL SECTION (= Lower part of Fig. 5 to enlarged scale).

Taking the latter as an extreme case, it may be regarded approximately as a "square-section" coil of length=depth =diameter of wire. As an example :—

A single turn of wire of mean diameter $D=10$ cm. and wire diameter=0·5 cm. :—

$$l=d=0{\cdot}5. \quad l/D=0{\cdot}05=d/D$$

$$\therefore\ k'=0{\cdot}1015.$$

$$\therefore\ L=\pi^2\times\frac{10^2\times1^2}{0{\cdot}5}\times0{\cdot}1015.$$

$$=200 \text{ cm.}$$

The actual inductance of this ring=192·2 cm.

To render this method of calculation more easily applicable to these cases, a series of values of k' for very small values of l/D and d/D have been calculated. They are set out in Fig. 8.

To determine the initial values of k (*i.e.*, $d/D=0$) required for these low-range curves, it should be noted that Rayleigh and Niven's formula for the inductance of short coils may be thrown into the same form as Nagaoka's. A comparatively easy method of calculation for the small values of k is thus obtained. Nagaoka's expression for this case is very cumbersome, and the values are outside his tables.

Rayleigh and Niven's formula is

$$L=4\pi aN^2\left[\log_e(8a/l)-\tfrac{1}{2}+\frac{l^2}{32a^2}\left(\log_e\frac{8a}{l}+\tfrac{1}{4}\right)\right]. \quad . \quad (8)$$

Equating this to $\pi^2D^2n^2lk$, we have

$$k=\frac{2}{\pi}\cdot\frac{l}{D}\cdot\left[\log_e\left(4\frac{D}{l}\right)-\tfrac{1}{2}+\tfrac{1}{8}\left(\frac{l}{D}\right)^2\left(\log_e\left(4\frac{D}{l}\right)+\tfrac{1}{4}\right)\right]$$

$$=\frac{2}{\pi}\cdot\frac{l}{D}\left[\log_e 2{\cdot}4261\,\frac{D}{l}+\tfrac{1}{8}\left(\frac{l}{D}\right)^2\log_e 5{\cdot}1210\,\frac{D}{l}\right]. \quad . \quad (9)$$

These low-range curves of k' do not extend to values of l/D less than d/D, for the reasons already given.

In conclusion, it is hoped that this indication of a uniform and easy mode of calculation of the inductance of all ordinary shapes of coils may be of some utility in practical work, by limiting the number of formulæ to be memorised or to become familiar with, and thus leading to less confusion and error.

ABSTRACT.

The method of calculation advocated in the Paper is based on an extension of Nagaoka's formula for single layer coils, to include as well all ordinary forms of thick coils. Rosa's formula for thick coils is put into the same from as Nagaoka's, and its use enables a series of correction factors to be calculated for various coil thicknesses. By the aid of a single sheet of curves giving values of these correction factors the inductance of any form of coil likely to be met with in practice may be readily calculated, using only one simple standard formula for all cases. Reasonable accuracy is obtained even in the limiting example of a single turn of wire. The results arrived at agree well with other published charts, which are usually of more limited application, while the use of a single formula for all cases lessens the liability to error.

It is also shown that Rayleigh and Niven's formula enables the calculation of the correction factors for very short coils to be carried out without having recourse to Nagaoka's more complicated expressions.

DISCUSSION.

Dr. ECCLES said that as the author had mentioned an abac given in the speaker's book on "Wireless Telegraphy," it was well to remark, first, that this abac was deliberately made of its present range so as to provide an open scale for coils of the proportions used in practice. For the rare cases of coils outside the range of the abac very simple formulæ are available. Dr. Russell's formulæ were used in making the abac, because they seemed more convenient for the purpose than the results of Nagaoka, to which they were, of course, equivalent. By constructing new curves for dealing with coils of several layers the author has rendered a great service, and has opened easy paths through a forest of laborious theoretical calculation. Now that this has been done it will be much easier to compare the measured values obtained on actual coils with the calculated values, and so we shall be aided to accumulate experience of the effects of high frequency eddy currents on the inductance of coils.

Prof. HOWE agreed with Prof. Eccles' remark about the general utility of Mr. Coursey's investigation. The chief difference between the author's curves and those of the Bureau of Standards was that in the latter the ratio D/l and k, which was a function of D/l, were combined into a single function.

Mr. NICOL referred to formulæ published in "The Electrician" by Mr. Coursey a few years ago, and considered that the step now taken, which resulted in restricting the values of k to between 0 and 1, combined with the general inclination of the curves to about 45 deg. with the axis, gave much greater convenience in reading.

XVI. *Some Characteristics of the Spark Discharge and its Effect in Igniting Explosive Mixtures.*

By CLIFFORD C. PATERSON, *M.I.C.E., M.I.E.E., and* NORMAN CAMPBELL, *Sc.D.*

RECEIVED FEBRUARY 19, 1919.

NOTE.—The work described in this Paper was carried out at the National Physical Laboratory at the instigation of the Advisory Committee for Aeronautics. The results have been communicated in a series of confidential reports to the Internal Combustion Engine Sub-Committee of that Committee, who have now given permission for the publication of any parts of it which appear of general scientific interest.

PART I.—THE NATURE OF THE SPARK.

[FOR SUMMARY SEE P. 196.]

INTRODUCTION.

1. *Objects of the Research.*

The work about to be described was undertaken with the ultimate object of determining whether any considerable improvement could be made in the electric ignition of explosive engines. The electric discharge used for this purpose is always produced either by a magneto or an induction coil, instruments which are essentially similar in principle and give discharges which probably do not differ very greatly in their fundamental characteristics. It was thought that a more complete study of the characteristics of the discharge which are necessary and sufficient to secure satisfactory ignition might possibly indicate that advantages could be secured by adopting some totally different system of producing the discharge. It appeared unlikely that the electrical advantages which any other system might be found to possess would be great enough to counterbalance the mechanical advantages of the present system, which has been so highly developed ; but in view of the possibility of such a discovery, it was decided to make the investigation as thorough and as fundamental as possible and not to confine the attention to discharges produced by methods similar to those used in practice.

2. *Previous Work on Electric Ignition.*

The study of the electric ignition of explosive mixtures has already a voluminous literature. Much of the work has been directed mainly to the chemical side of the problem, to the determination of the "limits of inflammability" in the variation of the proportion of fuel and air, and generally to the investigation of what mixtures will be ignited by a given sparking arrangement. The problem which is attacked in this research is rather what sparking arrangement will ignite a given mixture; on this problem light is thrown by a comparatively small proportion of the previous work. The following Papers (which will hereafter be denoted by the numbers attached to them) appear to contain most of the work that has been published in recent years which has any bearing on the present investigation.

(1) H. F. Coward, C. Cooper and C. H. Warburton. Chem. Soc. "Journ.," CI., pp. 2278-2287, December, 1912.

(2) H. F. Coward, C. Cooper and J. Jacobs. Chem. Soc. "Journ.," CV., pp. 1069-1093, April, 1914.

(3) W. M. Thornton. Roy. Soc. "Proc." A., XC., p. 272, 1914.

(4) W. M. Thornton. Roy. Soc. "Proc." A., XCI., pp. 17-22, November, 1914.

(5) W. M. Thornton. "Phil. Mag.," XXVIII., pp. 734-738, November, 1914.

(6) W. M. Thornton. Roy. Soc. "Proc." A., XCII., pp. 9-22, October, 1915.

(7) S. G. Gastry. Chem. Soc. "Journ.," CIX., 423-529, May, 1916.

(8) W. M. Thornton. Roy. Soc. "Proc." A., XCII., pp. 381-401, May, 1916.

(9) W. M. Thornton. "Phil. Mag.," XXXIII., pp. 190-196, February, 1917.

(10) R. V. Wheeler. Chem. Soc. "Journ.," CXI., pp. 130-138, February, 1917.

(11) R. V. Wheeler. Chem. Soc. "Journ.," CXI., pp. 411-413, May, 1917.

The explosive mixtures used in these experiemnts consisted almost always of hydrocarbons (usually pure), including hydrogen, with air or oxygen. Three different methods of producing the spark were employed. In (3), (5), (6) a "break spark" was used—that is to say, a constant current, direct or alternating,

flowing in a metallic conductor was interrupted by breaking the circuit at a point within the explosive mixture. In (1), (2), (7), (8), (9), (10), (11) the spark was produced across a constant gap by breaking the current in the primary of an induction coil, of which the secondary terminals were connected to the gap; the spark produced by this method is usually termed by the authors who used it "the impulsive electric discharge," or sometimes "the induction spark." In (4) and (5) a "condenser discharge" was used, produced by approaching two terminals, one connected to each coating of a condenser charged to a known potential.

Whichever method was adopted, it was always found that a spark could be passed through the mixture without causing ignition, provided that the spark was of sufficiently low "intensity." The experiments consisted in determining the least "intensity" which would ignite various mixtures. As a measure of this "intensity," necessary to express quantitative results, the value of the current broken was used in the case of the "break spark," the value of the current interrupted in the primary when the "induction spark" was used, and the values of the capacity of the condenser and the original potential to which it was charged when the "condenser spark" was employed.

The results obtained with the "break spark" and the "induction spark" have considerable direct practical importance—the former in determining the conditions in which explosions are likely to be initiated in mines, the latter in application to the coil ignition of explosive engines. But the latter, if not the former, are not easy to generalise; and it is still more difficult to co-ordinate either with the other or with the results obtained with the "condenser spark." For the critical "intensity" which will just explode a mixture certainly varies with the precise experimental arrangements employed. When the induction coil is used, the critical intensity will vary with the construction of the coil; when the "break spark" is used it will probably vary with the manner of break. Further, none but a purely empirical relation can be stated between the critical intensities for the same mixture measured by the three different methods of starting the discharge. Enough is not known of the mechanism of the discharge to formulate any general proposition about the relation between the discharge produced by breaking a certain current in the primary of an induction coil (even if the construction of the coil is fully

known) and that produced by approaching two terminals connected across a condenser of given capacity charged to a given voltage.

In other words, the result of all these experiments are stated in terms of the properties of the instrument used for producing the spark; they are not stated in terms of the properties of the spark itself, and it is doubtless the properties of the spark itself which determine its igniting power. That power must depend in some way on such characteristics of the spark as the average or maximum current through it, the time that it lasts, the potential between the terminals, the variation of that potential with the time, and so on. It is only if the igniting properties of the spark can be defined in terms of such characteristics that a result will be reached which is really general and permits a prediction of the igniting power of any spark produced by any experimental arrangement or a consideration of the best arrangement to produce a spark of given igniting power. To obtain such a result is the ultimate object of the research, and previous work does not give much help towards its attainment.

There are, however, a few general conclusions which may be deduced :—

1. With the induction and condenser sparks the critical intensity for a given mixture decreases if the spark potential is increased. In the break spark there is, of course, no spark potential. The spark potential in a given mixture and with given electrodes can only be changed by changing the sparking distance; few direct observations on the change of critical intensity with the spark gap were made, but all of them showed a marked decrease of critical intensity with increase of spark length. The spark potential can also be changed by altering the form of the electrodes, but the experiments are not sufficient to decide how the critical intensity varies with such changes. It can also be changed by changing the pressure of the mixture; and in this case the critical intensity again was found to decrease as the spark potential was increased. But since, for our purpose, a mixture of the same chemical composition at a different pressure must be regarded as a different mixture, such observations cannot be taken to show certainly that the critical intensity decreases with increase of spark potential in the same mixture.

2. The materials of the electrodes have little or no effect on the igniting power of the discharge. Thornton found some

effect due to the material of the electrodes, but his results have not been confirmed by others, and are capable of an explanation (to be discussed later) different from that which he puts upon them. In the "break" spark the material of the electrodes may have some effect, but since such a spark is really an arc, in which the discharge is conditioned by the temperature of the electrodes, this conclusion has no bearing on the true spark.

3. Much the most important conclusion to be drawn concerns the energy associated with the critical intensity. Among engineers the idea appears to have been widely prevalent that the energy dissipated in the spark was the determining factor in its igniting power. When this research was undertaken one of the most common methods of testing the efficiency of a magneto was to determine the energy dissipated in the spark which it gave between given electrodes in a given atmosphere; it appears to have been believed that the magneto which gave the greatest "joules per spark" would prove the most efficient igniting agent. The idea was not unplausible in the absence of any experimental evidence, and would naturally follow from any theory which regarded ignition as a purely thermal process. But the evidence contained in the papers cited is quite sufficient to show that the idea is erroneous. Most of the writers gave estimates (not always very reliable, but sufficient for our purpose) of the energy dissipated in the spark of critical intensity for a given mixture, and a comparison of their figures shows how widely this energy varies with the method by which the spark is produced. The energy of the critical break spark is often 100 times as great as that of the critical condenser spark in the same mixture. Moreover, Thornton's work enables the energy of different critical condenser sparks to be compared; he found that the energy, as well as the "intensity," of the critical spark decreased very notably as the sparking potential was increased.

It is curious that so many writers continue to express their results in terms of the energy of the spark when their own results show very clearly that the energy is not the determining factor in igniting power. The practice is probably due in part to the hope that, by means of the conception of energy, they may be able to express a relation between the igniting power of sparks produced by different means; if it were true that the igniting power of a spark is determined by the energy

in it, the problem of expressing igniting power in terms of the property of the spark, and not merely of the means of producing it, would be solved. But it is certainly not true, and a much deeper investigation is needed to discover what are the characteristics of the spark which determine its powers of ignition.

3. *Preliminary Experiments.*

It was thought at the outset that the two characteristics of the discharge which were most likely to determine its igniting power were the current density and the time that the discharge lasted. Accordingly an attempt was made to produce a form of discharge in which these two factors would be separately controllable. The known properties of the discharge at low pressure suggested that such control might be obtained, for at a pressure of 2 mm. or 3 mm. it is easy to find conditions such that the current carried by the discharge is a function of the form of the electrodes and the P.D. maintained between them, and remains constant as long as the P.D. is maintained. No evidence was known that similar conditions could not be obtained at much greater pressures, and it was expected that the chief difference which would be found between the discharge at atmospheric pressure and that at low pressures would be a reduction of the length scale of the discharge consequent on the reduction of the mean free path of the ions.

Accordingly the apparatus shown in Fig. 1 was set up, or, rather, part of it, for some of the portions which are shown and will be described later, were added subsequently.

S is the spark gap immersed in the explosive mixture ; the form of the terminals and the distance between them were varied in different experiments. In the preliminary experiments the explosive mixture consisted of hydrogen and air, and was always at atmospheric pressure.

Current was supplied to the spark gap from a constant source of potential. At first high potential dynamos were used for this purpose, but later a more convenient arrangement proved possible. It was soon found that the current which it was necessary to supply and the times which the current had to last were so small that the total quantity of electricity which passed in any one experiment was at most only a few micro-coulombs. Accordingly, the supply was taken from the condenser C_1 (a battery of Leyden jars with a capacity of about 0·1 mfd.), which could be charged up to any desired potential by a high-tension transformer, fed with alter-

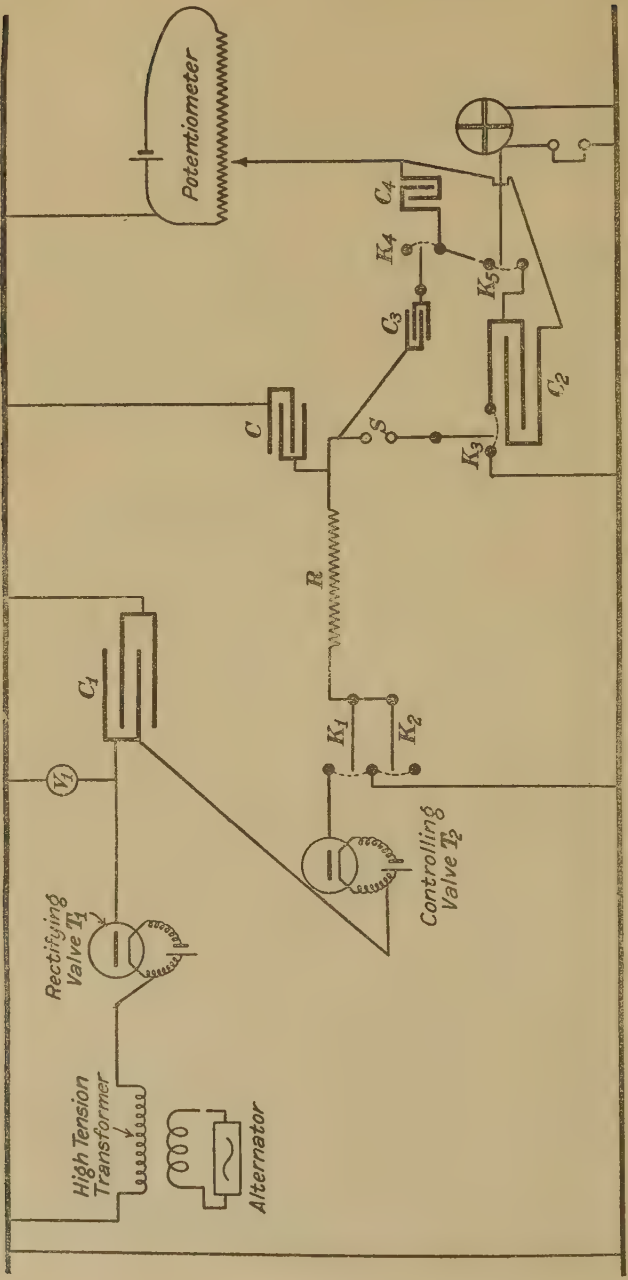


FIG. 1.—CONNECTIONS OF APPARATUS FOR TUNING SPARK.

nating current, and connected to the condenser through a thermionic valve T_1. The potential to which the condenser was charged was read by the electrostatic voltmeter V. So long as the total quantity of electricity which passes in a single experiment is a small fraction of that contained in C_1 when it is fully charged, C_1 can be regarded as a source of constant potential and used to replace the high-tension dynamos.

Between the source of potential and the spark gap were inserted devices which, it was hoped, would control the current passing through the spark and the time that it lasted. In order to control the current a second thermionic valve, T_2 was used. Since the valve was very highly evacuated, the current through it was approximately saturated and determined only by the temperature of the filament, and not by the P.D. across it. As a matter of fact, the saturation was not perfect; but the current only increased some 30 per cent. when the P.D. was increased from 500 to 10,000 volts, and it might be assumed with sufficient accuracy that the current passing through the valve was independent of such changes in the P.D. across it as were likely to occur when the discharge was started or stopped, so long as that discharge was of the nature which was expected.

In order to control the time that the discharge lasted the switches K_1 and K_2 were inserted. K_1 connected the valve to the spark gap, and so started the discharge; K_2 connected metallically the terminals of the spark gap, and thus stopped the discharge. These two switches, immersed in oil, were operated through electromagnets by a Helmholtz pendulum, which broke in succession two contacts, each of which short-circuited one of the electromagnets. One of these contacts could be moved relatively to the other by a micrometer screw, so that the time interval between the breaking of the contacts could be varied at will. The speed of the pendulum when it struck the contacts was 300 cm./seconds; the distance apart of the contacts could be changed by steps of 0·01 mm.; so that the time interval between the breaking of the contacts could be changed by steps of 3 micro-seconds. Owing to the delay in the action of the electromagnets, the interval between the operation of K_1 and K_2 was not equal to that between the breaking of the contacts by the pendulum; but, if everything else was unaltered, a change in the latter interval produced an equal change in the former.

The second terminal of the spark gap was connected to apparatus for measuring the quantity of electricity which passed through the discharge. One side of the condenser C_2 was connected to the spark gap through the switch K_3, and to the Dolazalek electrometer O; the other side was connected to earth through the potentiometer. If K_3 was closed, the electricity passing in the discharge was received in C_2, producing a deflection of the electrometer which could be compensated by means of the potentiometer. If v is the potential which must be imposed by the potentiometer to bring the needle back to zero, the quantity of electricity which has passed is C_2v. K_3 could be operated by the pendulum in place of K_2, but in the preliminary experiments it was always closed. The resistance shown at R was not present in these experiments.

4. *Preliminary Results.*

In taking observations the external potential, shown at V, was first fixed at some value considerably greater than the spark potential of the gap, and the filament temperature of T_2 fixed so that the current flowing through the valve had some value lying between 0·00001 and 0·1 ampere. Then, starting with the pendulum contacts in such a position that K_2 was known to operate before K_1, the operation of K_1 was advanced until a spark first passed across the gap when the contacts were operated by the pendulum. The corresponding reading of the micrometer screw should then correspond to the simultaneous operation of K_1 and K_2, and the interval corresponding to any other reading could be deduced.

It should be noted, in passing, that the eye could not be trusted to determine whether or no a spark had passed; the only safe criterion was the occurrence of a deflection in the electrometer; such deflection would often occur when no spark had been seen. The failure to see the spark was doubtless due to a wandering of the eye during the very short period during which it lasted. But when they are seen, the visibility of small sparks of very short duration presents some features of interest. As the time during which the spark lasted was increased, the apparent luminosity of the spark increased also, until the duration was so long that the spark appeared to the eye to last a finite time. After this stage was reached, an increase in duration did not produce any increase in apparent luminosity.

The object of the observations was to determine how long the spark, passing a known current, had to last in a mixture of known composition before ignition occurred.

The results which were obtained appeared at first puzzling. It was found that either the explosion occurred as soon as the spark passed at all or that it did not occur, however long the spark lasted; in a given mixture, which of the two alternatives occurred seemed to depend entirely on the temperature of the filament of the valve. If the temperature was gradually increased, the increase over a considerable range made no difference at all; however long the spark lasted—even if it lasted many seconds—no explosion occurred or any gradual combination, which would be shown by a decrease in the pressure of the mixture. But as soon as a certain limit was overstepped, the explosion occurred at the very first appearance of the spark and when it could not have lasted as long as 10 micro-seconds. The exact point at which the sudden change occurred varied considerably at successive trials, and varied also with the composition of the mixture, but the change was always perfectly sudden; no evidence whatever could be obtained that a discharge which would not cause explosion at the very first instant could cause explosion if it were maintained for any finite time.

The conclusion that ignition was so intimately connected with the starting of the discharge and that the discharge, if it did not cause ignition in the initial stages, would not cause it at all was not, perhaps, very surprising; several plausible explanations based on the process of ionisation, might be suggested. But it was very difficult to understand why, if the initial process of the discharge was all that mattered, the current flowing through the valve should make any difference. However, it is not worth while here to discuss all the speculations which were indulged in and all the fruitless experiments to which they gave rise, for the explanation was finally found to be so simple that it is now surprising that it was not expected from the outset.

Discontinuity of the Discharge.

The explanation was found as soon as systematic measurements were made of the quantity of electricity which passed through the spark gap to the electrometer. Two series of such measurements are shown in Fig. 2. The abscissæ are the time intervals corresponding to readings of the micrometer screw

on the pendulum ; one of these readings is arbitrarily selected as zero and the abscissa corresponding to any other reading is the time interval corresponding to the difference between that reading and the zero reading. The ordinates represent the quantity of electricity received in C_2.

Consider, first, the full-line in the figure. For values of the abscissa less than that represented by the point P no electricity passes, because K_2 is operating before K_1 ; P is the moment at which K_2 operates just after K_1, and times reckoned from P as zero are the times during which the high potential is applied to the spark gap.

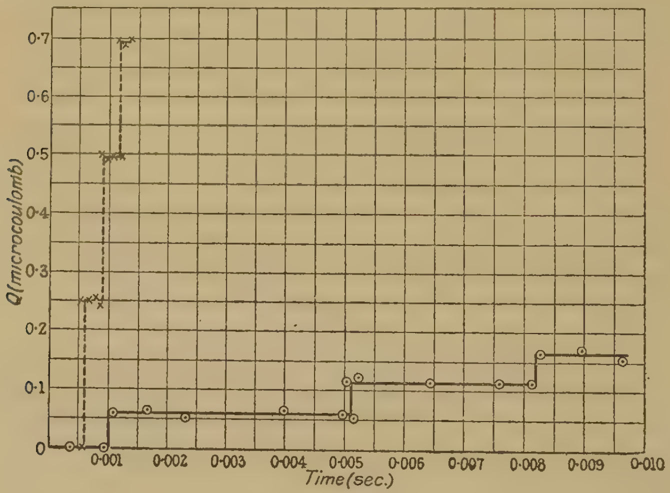


FIG. 2.

It will be seen that the quantity of electricity which passes across the gap does not increase continuously with the time that the potential acts ; the flow of electricity is discontinuous. A finite quantity passes at the moment P in a time so short that it cannot be measured by the pendulum ; an interval elapses during which no electricity passes, terminated by the passage once more of a finite quantity in a time which is inappreciable.

The significance of these observations is obvious. They show that the passage of the discharge in the circumstances of these experiments is the same as that which takes place when a Leyden jar with a spark gap in parallel is charged by a Wimshurst machine. The machine takes a finite time to charge the

jar up to the spark potential; a spark then passes in a time which appears infinitesimal to the eye; an interval then occurs in which the machine charges up the jar once more, and the whole process is repeated. Exactly the same cycle is happening in the conditions described, but the interval between successive sparks is too small for the eye to detect.

And now the fact that if the mixture is not exploded by the discharge at the first instance it is not exploded if the discharge is continued, is explained at once; it is merely evidence that if one of the series of sparks which constitutes the discharge is not able to explode the mixture, a succession of them at intervals long compared with the time that each lasts is not sufficient to cause ignition; the effects of one spark have died away before the next occurs.

It is not so immediately evident why a larger current through the valve should produce an explosion when a smaller current fails to do so; for at first sight it might be expected that the only effect of increasing the current would be to increase the rate at which the sparks pass; while, if the explosion occurs at the first spark, if it occurs at all, the rate of succession of sparks after the first should be immaterial. However, the dotted line in Fig. 2 shows that another factor comes into play. The measurements shown by this line was taken when the current which the valve would pass was about 50 times as great as in the previous observations. It will be seen that the interval between the separate sparks (*i.e.*, the successive steps in the line) is considerably reduced, but it is not reduced in the ratio of the currents; the successive steps in the ordinates are much greater with the larger current. The effect of increasing the current is to increase the quantity of electricity which passes in each spark as well as the rate at which the sparks pass. Since sparks in which different quantities of electricity pass may well have different igniting power, it is not surprising that the larger current should prove to have the greater igniting power.

But why is the quantity of electricity which passes in a single spark greater when the current through the valve is greater? To answer this question we must know what determines the quantity of electricity which passes. In the case of the Leyden jar charged by the Wimshurst, this quantity, Q, is (at least, very nearly) CV, where C is the capacity of the jar and V the spark potential. For the spark starts when the potential across the jar is V, and when it is over the potential is very

nearly 0; the discharge takes so short a time that the quantity of electricity supplied by the Wimshurst during that interval is quite inappreciable. But the relation $Q=CV$ will hold only so long as the quantity of electricity supplied by the Wimshurst during the time that the spark lasts is inappreciable compared with CV. If there were substituted for the Wimshurst some source of current which could supply electricity at a rate comparable with that at which it crosses the gap in the spark, then Q would doubtless be considerably greater than CV; indeed, if the rate of supply were so great that it was actually greater than the rate at which electricity could flow across the gap, we should expect the whole phenomenon to be changed, and the successive discontinuous sparks to be merged together to form one continuous flow.

Now, the valve T_2 limits the rate at which electricity can flow to the spark gap. We may imagine that what happens at the first moment of the discharge is that the electricity which has accumulated in the condenser formed by the portions of the apparatus between T_2 and the spark gap passes across the gap in a single rush occupying an inappreciable time. If the greatest current which can pass through the valve is so small that during this time the quantity of electricity which the valve can pass is small compared with that present before the discharge started, then the conditions will be precisely the same as in the case of the Wimshurst and Leyden jar. The discharge will stop when the quantity $Q=CV$ has passed, where V is the spark potential and C the capacity of that part of the circuit which lies between T_2 and the spark gap. If, on the other hand, the rate at which electricity can pass through the valve is so great that during the very short time occupied by the spark, a quantity of electricity can pass to the gap which is not small compared with CV, then we should expect Q to be greater than CV. (*See* p. 191.)

In this manner we can explain qualitatively the effect of the filament temperature of T_2 (determining the greatest current which can pass through the valve) in determining the value of Q, the quantity of electricity which passes in a single spark, and consequently, the igniting power of that spark. In order that the whole matter should be adequately cleared up, it is essential that quantitative measurements should be found to be in accord with the theory suggested. A complete proof would be provided if it could be shown that, so long as the current through the valve were sufficiently small, Q is actually equal

to CV, where V is the spark potential and C the capacity of the circuit to earth between the valve and the spark gap; and further, that i_0, the value of the current through the valve at which Q becomes greater than CV, is such that $i_0 T$ is comparable with CV, where T is the duration of the spark. But neither of these experiments is easy to perform with the apparatus unaltered; for one thing it is very difficult to estimate at all accurately the capacity of such a complicated circuit as that which lies between T_2 and the spark gap. But in one respect such measurements as could be made were in accordance with the theory. Thus, it was found, as the theory predicts, that so long as the current was less than a certain value, Q was independent of the current. On the other hand, the value of the current at which Q began to increase was considerably less than that which would be expected, if for T is taken the maximum value fixed by the fact that observations such as those of Fig. 2 show that this time is inappreciable by the pendulum. More will be said of this discrepancy later.

5. *The Measurement of Q.*

If the explanation suggested is correct, any other device which prevents the current passing through the spark gap rising above the necessary limit during the passage of the spark should act in the same manner as the valve, and should limit the portion of the circuit which discharges through the spark to that between the limiting device and the spark gap. Accordingly, the effect was tried of inserting between T_2 and the spark gap a high resistance R in the position shown in Fig. 1. If the resistance of R is sufficiently great the value of Q when the spark passes should be determined by the capacity of the portion of the circuit between R and the spark gap, but should be independent of the capacity of the portion between R and T_2. This expectation was found to be fulfilled. If a condenser was inserted in parallel with the spark gap in the position C_3 of Fig. 1, then Q was increased whatever the value of R; but if the condenser were inserted between R and T_2, then, if R were sufficiently great, its presence had no effect upon Q. The limiting value of R, such that it was just great enough to prevent a condenser inserted between T_2 and R affecting Q, was found to be of the oder of 10 megohms; it would doubtless vary considerably with the exact conditions of the experiment, with the spark potential and possibly

with the form of the electrodes or the nature of the gas in which the spark passes.

The high resistance R was a small glass tube filled with a mixture of xylol and alcohol, and provided with platinum electrodes. The capacity of the resistance itself was less than 1 mmf. (1 mmf. $=10^{-12}$ farad), and small compared with that of the remainder of the spark gap circuit. Accordingly there was now no difficulty in determining the capacity of the portion of the circuit which discharges in the spark, for the connection between R and the spark gap could be broken, and the discharging portion completely insulated without any material change in its capacity. The capacity, in the absence of any added condenser, was determined by a simple modification of the ordinary method of mixtures; the circuit was charged to 500 volts by a battery, and then, by means of two

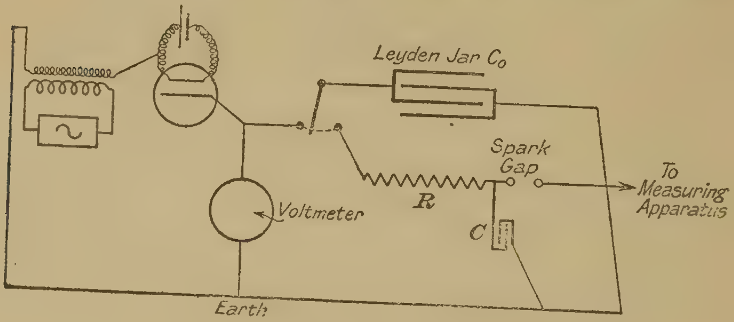


FIG. 3.

contacts worked at an interval of about 0·001 second by means of the pendulum, it was successively insulated and connected to a condenser of known capacity with the electrometer in parallel. The capacity of the discharging circuit could be increased by inserting condensers of known capacity in the position C_3.

A modification was also made in the method by which the number of sparks which pass across the gap was limited. In the arrangement shown in Fig. 1, the limitation is effected by means of the pendulum, which limits the time during which the high potential is applied to the spark gap; in the new arrangement it is effected by limiting the total quantity of electricity which is available. The connections are shown in Fig. 3. The Leyden jar C_0 is first charged up to any desired

potential, indicated by the voltmeter, by means of the transformer acting through the valve; by means of the throw-over switch it is then connected across the resistance R and the spark gap in series. If V is the spark potential of the gap, C_0 the capacity of the Leyden jar, C that of the spark gap circuit, then, if the Leyden jar is initially charged to a potential $V+v$, the potential across the gap, after the switch has been thrown over, will be $C_0(V+v)/(C_0+C)$. If this is greater than V, sparks will pass across the gap, each carrying away the quantity of electricity $Q=CV$, until the potential across the gap has fallen to less than V. If n is the number of sparks that pass, n is the integer next less than $C_0v/CV-1$. Accordingly by adjusting v, n can be given any desired value.

The first measurements which were made were directed to determine how far it was accurately true that $Q=CV$, where Q is the quantity of electricity which passes across the gap in a single spark, V the spark potential, C the capacity of the spark gap and any condensers in parallel—*i.e.*, of that part of the circuit which is effectually isolated from the remainder during the passage of the discharge by the high resistance R.

The results of these measurements are given in Table I. The value of V at the head of each section is the spark potential of the gap determined in the ordinary manner with a steady potential. The first column gives the measured value of C, the second the product CV, the third the measured value of Q. If $Q=CV$, the numbers in the second and third columns should be the same. However, since great care was not taken to adjust v so that n was always 1, sometimes two or three sparks may have passed and not only one; hence, it is to be expected that the numbers in the third column should be nCV, where n is usually 1, but may be 2 or 3.

It will be seen that in the first section of the table, the agreement between the second and third columns is remarkably good; differences in almost all cases lie well within the anticipated error of observation. The agreement persists in the earlier parts of the two later sections, but in these sections, when C is increased beyond a certain limit (less for the greater spark potential), numbers appear in the third column which are markedly different from those in the second; in some cases they are actually different in sign. These discrepant observations are shown in italics.

The occurrence of observed values of Q with a reverse sign is not very easy to understand. It is not surprising that

there should be, at some stages of the discharge, a current across the gap opposite in direction to that determined by the initial field; for it is to be expected that the discharge

TABLE I.

$V=3100$ volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·050	$2\times0{\cdot}052$, 0·050, $2\times0{\cdot}053$, 0·056.
21·2	0·066	0·072, 0·072, 0·069.
24·4	0·076	0·084, 0·087, 0·081.
32·6	0·101	0·106, 0·103, 0·109.
46·8	0·145	0·150, 0·144, 0·147.
71·8	0·223	0·228, 0·230, 0·228.
121·0	0·376	$2\times0{\cdot}374$, $2\times0{\cdot}348$, 0·396, 0·396, $2\times0{\cdot}404$.
177·0	0·550	$2\times0{\cdot}503$, 0·561, 0·561.
225·0	0·700	0·692, 0·673, 0·702.
346·0	1·072	1·04, 1·07, 1·06.

$V=4230$ volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·068	0·069, $2\times0{\cdot}069$, 0·065.
21·2	0·090	0·090, 0·090, 0·087.
24·4	0·103	0·103, 0·103, 0·100.
32·6	0·137	0·134, 0·134, 0·137.
46·8	0·197	0·187, 0·184, 0·187.
71·8	0·303	0·303, 0·294, $3\times0{\cdot}286$, 0·294, 0·296.
88·4	0·372	$3\times0{\cdot}323$, 0·372, $3\times0{\cdot}320$, 0·372.
103·0	0·435	1·22 ($=3\times0{\cdot}407$?), *0·694*, *2·10*.
121·0	0·511	0·538, 0·519, *−0·121*, 0·528, *0·728*, 1·50. ($=3\times0{\cdot}50$?).
138·0	0·583	0·607, *4·23*, *−0·719*, *3·62*, *0·820*, *0·017*.

$V=5350$ volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·086	$2\times0{\cdot}086$, 0·090, 0·094, $2\times0{\cdot}087$, 0·094.
21·2	0·114	0·118, $2\times0{\cdot}115$, 0·118.
24·4	0·131	0·137, 0·134, 0·134.
32·6	0·175	0·172, 0·168, 0·166.
46·8	0·251	0·256, *0·340*, *0·106*, *0·066*, *0·375*.
60·7	0·332	*−0·451*, *0·107*, *2·17*.

should be oscillatory. But, if current flows nowhere except between the two poles of the spark gap, a charge left finally on the low potential side of the gap opposite in sign to that

originally present on the high potential side must indicate that the high potential side has increased its potential, and that the final field across the gap is greater than the initial field. Since this conclusion must be erroneous, the assumption on which it is based must be wrong, and current must flow elsewhere than between the two poles of the gap ; for instance, it may flow from the high potential terminal to earthed conductors in the neighbourhood of the gap. And such an occurrence is not impossible, for the "explosive action" of the spark may carry some of the ions to a considerable distance, so that they lose their charge on conductors other than the terminals of the gap. If part of the charge originally present on the high potential terminal is thus given to other conductors, a charge of reversed sign left finally on the low potential side of the gap might be explained. It should be observed, therefore, that the discrepant observations occur when the energy dissipated in the gap is increased by increasing either the spark potential or the capacity discharging ; the greater the energy dissipated, the greater is the explosive action of the spark, and the more likely that part of the charge originally present is carried to conductors other than the terminals of the gap.

It is obvious that this view might have been tested by experiment, but since the primary object of the research was not to investigate in detail all the processes occurring in the spark, but only those which are likely to determine its power of igniting explosive mixtures, further investigation in this direction was not undertaken. The experiments sufficed to show that under certain conditions the quantity of electricity which passes across the gap is simply the product of the spark potential and the capacity discharging. This conclusion, which, of course, might have been anticipated from the start, was all that appeared necessary for the later experiments.

But one further observation should be noted. In all these experiments Q has been measured for the first spark which passes. It is interesting to inquire whether, if the first spark is followed by a stream of others in rapid succession, the later sparks are characterised by the same value of Q as the first. It was found that Q was in general less for the later sparks, if they followed each other sufficiently rapidly. Some indication that Q is smaller for the second and succeeding sparks may be obtained from Table I., but more definite evidence is shown by Fig. 2. It will be seen there that the

second and succeeding steps in the line are smaller than the first. The explanation of this difference probably lies in the fact that V, the spark potential, is less for the second spark than for the first. It is well known that the potential necessary to maintain a spark across a given gap is somewhat less than that needed to start it; the difference is probably due, in part at least, to the decrease in the density of the gas surrounding the spark gap by the passage of the first spark.

6. *The Duration of the Spark.*

Attempts were made to determine the duration of the spark, but they led to no definite result, and gave only an upper limit to the possible duration.

It was thought that if the contact K_2, which short-circuits the spark gap, could be closed during the actual passage of the spark, the discharge across the gap would be stopped, and the remainder of the charge on the high potential side diverted to earth through the contact, the quantity of electricity received by the low potential side of the gap would then be less than the normal amount. However, all attempts to obtain by variation of the interval between the operations of the contacts K_1 and K_2, values of Q which were less than the normal amount proved fruitless; either no spark passed at all, and Q was 0, or a spark passed and the full value of Q equal to CV was obtained. No intermediate value was recorded even when the conditions were such that a change in the interval between the operations of the two contacts of less than 0·00001 second produced consistently a change from $Q=0$ to $Q=CV$. At first sight these observations would seem to show that the time occupied by the spark must be very small compared with 0·00001 second. It is quite possible that this conclusion is correct, but further consideration shows that it is not established by the experiments. However small the time occupied by the discharge, it would be expected that in a large number of experiments (and over 300 were made in this connection) chance would decide that in one of them K_2 would operate during that time; the failure ever to observe a value of Q intermediate between 0 and CV suggests that it is impossible by the operation of the switches to stop the spark once it has started discharge across the gap. And further consideration provides an explanation of the impossibility. When K_2 closes, it does so first by means of a spark across the approaching contacts. Sup-

pose, then, that the interval between the operation of K_1 and K_2 is such that K_2 sparks just before the spark potential of the main gap is reached, so that no spark passes across that gap; then the distance between the contacts of K_2 when the spark passes will be just less than that corresponding to that spark potential. If the action of K_2 is now delayed very slightly, the spark will have started across the main gap before K_1 reaches the position in which it previously sparked; owing to the passage of the spark across the main gap the potential will have fallen, and no spark will pass across the contacts of K_2 in this position. Further, no spark will pass at all across the contacts of K_2 unless the approach of those contacts is so rapid that the diminution of the spark potential between them is more rapid than the fall of potential across the main gap due to the passage of the discharge. Accordingly the failure ever to stop the spark once it has started by the operation of K_2 only shows that the rate of approach of the contacts was not sufficiently rapid. The known rate of approach and the variation of the spark potential with the separation of the contacts admits of a rough maximum estimate being made of the duration of the discharge; this maximum estimate turns out to be about 0·00005 second.

An attempt was also made to perform the same experiment in a rather different way. In place of using the second contact of the pendulum to short-circuit the spark gap, it was used to break connection at K_3 (Fig. 1) between the low potential side of the gap and the measuring apparatus. Again, it would seem that if such a break could be effected during the actual passage of the spark a value of Q would be recorded greater than 0, but less than the normal value. But this attempt also failed; values of Q other than 0 and CV could be recorded, but they were wholly irregular, sometimes greater than CV and sometimes of the reverse sign. The anomalies were traced partly to the fact that the charge received by the measuring apparatus requires a finite time to reach its ultimate distribution among the various parts of the apparatus; this time is apparently of the same order as that occupied by the discharge. But it was also suspected that, if the break were made during the actual progress of the spark across the main gap, a discharge took place across the separating contacts; for it must be remembered that, though the whole quantity of electricity transferred is very small, the current which flows during the actual time of passage may be considerable.

Accordingly, direct measurements of the duration of the spark gave no definite result; an indirect estimate yielded no more satisfactory information. The explanation which has been put forward for the effect of the high resistance R in limiting the quantity of electricity which passes in the single spark indicates that the value of R must be so great that the quantity of electricity which passes through it, while the sparks lasts, must be small compared with that accumulated on the high potential terminal before the discharge begins. This condition involves that CR, where C is the capacity which discharges, shall be large compared with T, the duration of the spark. Now, when C was 25 mmf., it was found that R had to be greater than 10 megohms, in order that the necessary condition should be fulfilled. CR is thus 0·00025 second, and T should be less than this value. The previous conclusion was that T could not be greater than 0·00005 second, or one-fifth of CR. There is thus no certain conflict between the two estimates, but on the other hand, since the previous estimate is nothing but a maximum estimate, and may exceed very greatly the true value, it can hardly be said that the indirect method confirms the direct. It may be pointed out in this connection that it is not certain that the T, which must be small compared with CR, is the same quantity as that which it was attempted to measure directly. The latter is the time during which current actually flows across the gap; the former is the time after which another rise of potential will not produce any current until the spark potential is reached once more. Now it is quite possible that, even after the discharge has ceased, strong residual ionisation may remain between the terminals of the gap which will permit an appreciable transfer of electricity at potentials far less than that required to initiate the discharge.

It will be observed that, according to these considerations, the value of R necessary to limit the discharge to the portion of the circuit between the resistance and the spark gap should decrease as the capacity C increases, if the duration of the spark is independent of the capacity. A few rough observations certainly showed that the necessary value of R does decrease as C increases, but sufficient measurements were not made to obtain any information as to the probable variation of T with C.

The only conclusion, then, that can be certainly based on these experiments is that the spark does not last as long as

0·00005 second. But there is some reason to believe that this maximum estimate is considerably greater than the true value. For it seems probable that the duration of the very feeble sparks, discharging very small capacities, with which we are here concerned would be less rather than greater than the duration of strong sparks due to the discharge of large Leyden jars. Now a limit is set to the duration of such strong sparks by their use in illuminating rapidly moving objects; familiar illustrations—*e.g.*, of moving rifle bullets—shows that the light from such strong sparks can certainly not last as long as 0·00001 second. But it must be remembered that the period for which the light lasts is not necessarily identical with that during which an appreciable current flows. On the other hand, observations of a quite different character would seem to set a lower limit to the duration of such strong sparks. The effective resistance of such sparks has been estimated from the damping which they produce in oscillating circuits with frequencies of the order of 10^6. It would seem that, in order to obtain self-consistent measurements of the resistance by such a method, the spark must last for a time comparable with the period of the oscillations, and therefore that the duration cannot be much less than 0·000001 second. But, once more, it is very doubtful whether these estimates can be applied to the very much feebler sparks which with these observations were concerned.

7. *The Limits of the Discontinuous Discharge.*

It will be seen that the investigation diverged widely from the scheme that had been planned at the outset. It had been expected originally that it would be possible to obtain and control a continuous discharge of which the duration could be readily varied, but the only form of discharge that was found was discontinuous, consisting of a succession of individual and indivisible sparks, the duration of which could not even be measured, far less controlled. For the purpose for which the research was undertaken this failure of expectation was unimportant; the object was to investigate the igniting power of such discharges as are likely to occur in practice, and when it was known that all such discharges were discontinuous, the investigation of continuous discharges ceased to be of immediate interest. Nevertheless, it is well to consider briefly how far the results which have been attained

indicate that in any conditions a continuous discharge in air at atmospheric pressure is likely to be obtainable.

The experiments seem to show that a continuous discharge could only be obtained if the rate of supply of electricity to the spark gap could be made so rapid that, during the very short time for which the individual spark lasts, a quantity of electricity nearly equal to that passing in each spark flows to the gap. If this condition could be obtained, it is to be expected that the successive individual sparks would merge into each other, and some form of continuous discharge obtained. In order to fulfil this condition, one or two alterations in the circumstances of the experiment must be made; either the current passing to the spark gap must be increased, or the quantity of electricity passing in each spark must be decreased. It is interesting to inquire whether by making changes in either of these directions it is possible to obtain a discharge in which measurements similar to those shown in Fig. 2 will give a continuous line, indicating that the flow of electricity across the gap is continuous, and does not take place in a series of isolated sparks.

It is difficult to reduce very much further the value of Q, the quantity of electricity passing in each spark. For reasons which will be given in a later communication, it is scarcely possible to reduce V, the spark potential, below 2,000 volts, while C, the capacity, cannot easily be reduced below 10 mmf. The least value of Q which is possible for the apparatus as designed is therefore 2×10^{-8} coulomb. If T, the duration of the spark, is not greater than 0·00005 second, the current supplied to the gap must be at least 0·0004 ampere before fusion of successive sparks is to be expected. The value of R necessary to isolate the discharging capacity increases as Q decreases; with so small a Q, R would have to be 100 megohms. Now, to drive 0·0004 ampere through 100 megohms requires 40,000 volts; and such a potential would be quite impossible to control by the means hitherto adopted.

This difficulty might be avoided by "isolating" the discharging capacity by a valve instead of by the high resistance; the potential required with that arrangement is only slightly greater than the spark potential of the gap. But when the valve is employed it is necessary to insert the arrangement for controlling the time that the discharge lasts between the valve and the spark gap, and the capacity of that arrangement is included in that which discharges in the spark. Under these

circumstances it is not possible to reduce C below 60 mmf.; Q cannot be less than 12×10^{-8} coulomb, or the current less than 0·0024 ampere. But when so large a current is passed through the valve, the stage, mentioned on p. 180, is reached, at which the valve ceases to isolate effectively the part of the circuit between it and the spark gap. Although the discharge which still passes consists of individual sparks separated by intervals in which no current flows, the value of Q for those sparks is considerably greater than CV, and the stage at which the coalescence of successive sparks is to be expected is still further postponed.

For these reasons it was not found easy to follow experimentally the passage of the discontinuous into the continuous discharge; indeed, it was not certain that a continuous discharge could be obtained at all, until the stage was reached in which an arc occurs. It is clear that, if the current and, therefore, the energy dissipated in the discharge are increased sufficiently, a condition must ultimately be reached in which the electrodes will become sufficiently heated for the discharge to pass into the arc determined by thermionic emission from the kathode. Continuous discharges could certainly be produced by increasing the current passed by the valve, but it appeared that they were of the nature of arcs rather than true independent discharges.

There is another way in which a continuous in place of a discontinuous discharge can be obtained—namely, by a change in the form of the electrodes. In all the experiments described here the electrodes were such that the radii of curvatures of their surfaces were large compared with the sparking distance. If, on the other hand, the sparking distance is large compared with the radius of curvature of the electrodes, it is possible to get the brush discharge with a P.D. less than that required to give the discontinuous spark. It is probable that if more finely pointed electrodes had been used it would have been found that the coalescence of successive sparks into a continuous discharge would have occurred for smaller values of the current, and the process might have been observed experimentally. But experiments were not extended in this direction, and it was subsequently found that much of the work which might have been attempted had already been described in an interesting, though a somewhat diffuse, paper by Max Toepler ("Ann. d. Phys.," II., 7, 560-635, 1900). This writer has examined the conditions

which determine the passage of the discharge from the brush to the discontinuous spark, and thence to the arc, as the current is increased; and he found, in accordance with the indications of the work that has just been described, that the region in which the continuous discharge was possible was narrower the greater the capacity in parallel with the spark gap.

8. "*Capacity*" *and* "*Induction*" *Sparks.*

The observations that have been recorded show that the sparks which have been investigated were essentially similar to the "capacity" or "condenser" sparks of the introductory paragraphs produced by the discharge of a charged condenser. It is interesting to consider what bearing, if any, they have on the nature of the "induction" spark.

If the spark produced between the secondary terminals of an induction coil when a current is broken in the primary is observed in a rotating mirror or photographed on a rapidly moving film (*), it is found to consist of a succession of individual sparks separated by intervals in which no discharge is seen. The first spark is always much brighter than those which follow. The sparks after the first are separated by equal intervals, which correspond to time intervals equal to the period of the oscillation of the secondary circuit; the interval between the first spark and the remainder is not in general the same as that separating the later sparks.

The question arises whether these sparks, like those investigated hitherto, are characterised by the passage across the gap of a definite quantity of electricity, and, if so, by what this quantity is determined. On analogy with what has been found already we should expect that the first spark, at least, would convey a quantity of electricity Q, equal to the product of V, the spark potential, into some capacity C; and at first we should be inclined to identify this capacity with the capacity of the secondary circuit as determined by measurements of the oscillation frequencies.

Some investigations in this direction were begun, but were not completed, when more urgent problems claimed attention; nevertheless, the preliminary results were of sufficient interest

* The structure of the spark produced by a magneto, which, of course, is an "induction" spark, may be investigated very conveniently by means of the "rotary spark gap," which has been developed for investigating the timing of magneto sparks. The bright first spark, followed after a definite interval by much fainter sparks equally spaced, or sometimes by a continuous "flame," is very clearly seen.

to deserve record. In these experiments a current flowing through the primary of a small induction coil was broken by the first pendulum contact; the secondary terminals were connected to a spark gap which was short-circuited, as before, by the operation of the second contact. The low potential side of the gap was connected, as before, to the apparatus for measuring the quantity of electricity which had passed across the gap. By varying the interval between the operation of the pendulum contacts the quantity of electricity which passed during known intervals after the break of the primary circuit could be measured.

It was found, as was to be expected, that the relation between the interval between the operation of the contacts and the quantity of electricity received was of the same form as that shown in Fig. 2. As soon as a spark passed at all, a finite quantity of electricity was received in a time which could not be divided by the pendulum; then an interval occurred in which no further charge was received by the measuring apparatus. The quantity of electricity passing in the first spark was a perfectly definite quantity, and its value could be determined consistently at successive trials so long as the apparatus remained unchanged. After a certain interval after the first spark, a stage was reached at which a further quantity of electricity was received by the measuring apparatus, indicating the passage of a second spark of some kind, but the preliminary observations showed that the quantity passing in this subsequent discharge was much less regular than that passing in the first spark; consistent measurements could not be obtained in the conditions employed. Accordingly, attention was paid only to the first spark.

It was found immediately that the expectation that Q would be equal to CV, where C is the secondary capacity, was not fulfilled. In the first place, although the spark gap (and, therefore, presumably V) was unchanged, Q increased rapidly with the current broken in the primary. This conclusion might have been anticipated without any detailed measurements, for it is a familiar observation that the "fatness" or luminosity of the spark from an induction coil, the gap being unchanged, increases rapidly with the current broken in the primary. In general, a greater luminosity indicates that a greater quantity of electricity passes across the gap in the spark. Now, the secondary capacity of an induction coil is a

"distributed" capacity, and therefore a somewhat indefinite quantity, but there is no indication that it varies with the primary current; for the period of the oscillations excited (which is determined in part, by the secondary capacity) does not vary with that current. Accordingly, if we are to write $Q=CV$, the C in this equation is not the secondary capacity as estimated by oscillation methods.

Again, if $Q=CV$, Q should increase when the sparking distance and the sparking potential are increased; but

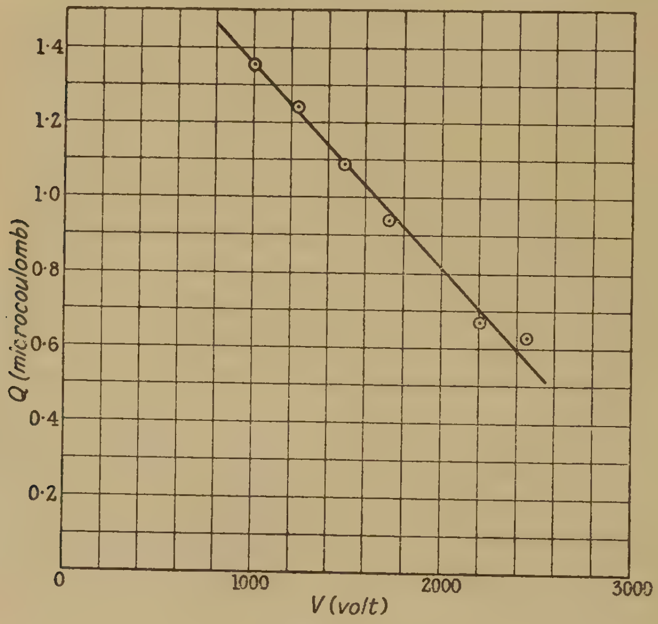


FIG. 4.

measurements show that it decreases. Fig. 4 gives the relation between Q and V in one series of experiments, in which the current was the same throughout. In no respect, therefore, is there even qualitative agreement between theory and experiment, if we make C a constant quantity and identify it with the normal secondary capacity of the coil. Nor is there quantitative agreement. The secondary capacity of the coil was not actually determined, but it is known that it could not have been as great as 30 mmf.; whereas the lowest value determined from the relations $Q=CV$ (namely, with the

greatest sparking distance across which a spark could be obtained with a current of 0·5 ampere) was 300 mmf.

It is clear then that the quantity of electricity which discharges in the induction spark is not simply determined by the capacity of the spark circuit; and further consideration shows that this conclusion is not surprising. For the normal secondary capacity applies only to the condition when the secondary terminals are insulated externally; when the spark starts they are joined by a conductor, and the presence of this conductor is equivalent, so far as determination of the period of the oscillations is concerned, to the addition of a condenser of large capacity. Indeed, the fact that the passage of the spark makes the secondary circuit for the time being a closed conducting circuit suggests that a quite different method of calculating Q is worthy of consideration. If, when a current, i, is broken in the primary, the secondary terminals are joined by a metallic conductor, so that the total secondary resistance is R, then, by a well-known theorem, the total quantity of electricity which will pass round the secondary circuit as a result of the break of the current in the primary is iN/R, where M is the mutual induction of the primary and secondary circuits. Now, the spark is such a conductor joining the terminals of the secondary, and, if we knew what value to attribute to its effective resistance we might calculate Q from the formula just given; or, conversely, we might use the formula to calculate the effective resistance of the spark. Unfortunately, however, it does not seem that this method is legitimate, for the Q which is equal to iN/R is the quantity which has passed round the circuit after a time so long that all disturbances excited by the break in the primary have died away; this time is certainly not less than the period of oscillation of the coupled circuits. But the spark does not last more than a fraction of that period. Nevertheless, though the formula cannot be applied strictly, it provides a qualitative explanation of some of the relations previously noted; thus, a decrease of the spark gap might be expected to produce a decrease of the effective resistance of the spark, and we actually find that it produces an increase of Q, and again Q increases with i.

The relation between Q and the characteristics of the gap and of the coil certainly deserves further consideration both from the theoretical and the experimental side. The results which have been given suffice to show that, in any given conditions, the spark produced by the induction coil, like that

produced by the discharge of a condenser, is characterised by a definite value of Q. This is the result which appeared important for the main purposes of the research. For if it be true that capacity and induction sparks are both characterised by a common feature, namely, that in both the passage of the spark is accompanied by the passage of a definite quantity of electricity across the gap, the relation which it was hoped to establish between the two forms of discharge has been found. It is to be expected that if the electrodes and the sparking distance are the same, the igniting power of the spark will be determined by Q; and that two sparks, produced one by an induction coil and the other by the discharge of a condenser, will have the same igniting power if for both of them the value of Q is the same. Of course, experiment will be necessary to establish that proposition; it is still possible that there might be a difference, as, for example, in the time that the spark lasted, which would make a difference in the igniting power, even if Q were the same.

Summary.

1. The object of the investigation was to determine the relation between the electrical characteristic of a spark discharge, and its power of igniting explosive mixtures.

2. Previous work on the subject is briefly reviewed. Certain general conclusions appear to have been established, and in particular it has been shown that the energy dissipated in the discharge is not the factor of prime importance in determining its igniting power. But most of the work relates to igniting power to the properties of the apparatus by which the discharge was produced rather than to the properties of the discharge itself.

3. The scheme of the research is explained. An attempt was made to produce a form of discharge in which the current passing and the time for which it lasted could be controlled and varied.

4. Preliminary observations show that the attempt to obtain such a discharge had failed. The discharges obtained always consisted of a discontinuous series of individual sparks, each of which lasted for a time which could not be sub-divided.

5. Quantitative measurements show that each of these individual sparks consists in the passage of a definite quantity of electricity Q across the gap, and represents the discharge

of a condenser of definite capacity previously charged to the spark potential of the gap.

6. Attempts to determine the duration of each of the individual sparks only led to a maximum value being assigned to that duration. This maximum is 0·00005 second; there is reason to believe that the true value is considerably less than this maximum estimates.

7. The limits to the conditions in which the discharge will be discontinuous and of this form are considered. It was not found possible experimentally to obtain a discharge which was continuous, except when it took the form of an arc or a brush.

8. Some preliminary observations on discharges produced by an induction coil are described. It is shown that such sparks, like those obtained by the discharge of a condenser, are characterised by the passage across the gap of a definite quantity of electricity. It is suggested, therefore, that this quantity, together with the form of the gap, may be sufficient to define the nature of the spark and to determine its igniting power.

The second part of the Paper will deal with observations on the igniting power of spark discharges.

PART II.—IGNITING POWER OF SPARK DISCHARGE.

9. *Plan of the Research.*

It has been established in the preceding part of this Paper that the true spark discharge is discontinuous and consists of a succession of individual sparks; each of these sparks lasts a time so short that it has been found impossible to measure it, and each is associated with the passage across the spark gap of a definite quantity of electricity determined on the one hand by the spark potential, on the other by the nature of the source of potential and of that part of the circuit which connects the source to the spark gap. The individual sparks are essentially similar to the spark which occurs when a condenser is raised to the spark potential of the gap and then allowed to discharge across it; but the capacity of the condenser which gives the equivalent spark in such circumstances is determined by the characteristics of the remainder of the circuit.

The "capacity" spark, then, seems to represent the normal type of spark, and sources of supply which appear essentially different from that by which the capacity spark is usually produced give discharges which differ from the capacity spark only in the fact that they give a succession of sparks (which may or may not all be similar) in place of a single spark. If this conclusion is correct it follows that, if we investigate thoroughly the igniting power of capacity sparks, we shall be able to predict the igniting power of any form of spark by whatever agency it is produced. A considerable amount of information concerning ignition by a capacity spark, produced directly by the discharge of a condenser in the ordinary way, is already available from the work of Thornton. but his observations were limited to those in which the capacity of the condenser discharged was large (more than 1 mf.) and the spark potential small (a few hundred volts). The extension to smaller capacities, and therefore, if ignition is to ensue, larger spark potentials would have proved difficult, if no way of producing a capacity spark had been available other than that of first charging a condenser to the requisite potential and then approaching its terminals; by such a method it would obviously have been difficult to extend the observations to capacities as small as a few micro-microfarads. But the experimental arrangements described in § 5 enabled such small capacities as these to be used with ease; we have only to isolate the small capacity from the rest of the circuit by means of a sufficiently high resistance and then apply a steady potential; a stream of sparks will then flow, each representing the discharge across the gap of the small capacity charged up to the spark potential.

Whether or no a given spark will ignite a given mixture will probably depend on the following characteristics :—

(1) The nature of the mixture, *i.e.*, its chemical composition, pressure, temperature, and, possibly, state of motion and electrical characteristics. In what follows, unless the contrary is stated, mixtures which differ in any of these respects will be regarded as different mixtures, *e.g.*, mixtures of the same composition at different pressures.

(2) The capacity which discharges across the gap in the spark.

(3) The nature of the gap—*i.e.*, the form and material of the electrodes and the distance between them. These characteristics determine the spark potential in conjunction

with (1); the spark potential in conjunction with (2) determines Q, which is the quantity of electricity which passes across the gap in the single spark.

In the experiments about to be described little attention is paid to (1). Mixtures of various natures have been investigated, but the variation has been determined either by experimental convenience or selected as a means to investigating the effect of (2) and (3). In all the experiments (2) has been varied within wide limits; in those of the first series the form of the electrodes was unchanged, while the spark potential was varied by changing the distance between them. In a later series the effect of the form and material of the electrodes is considered. The main object has been to discover laws relating the electrical characteristics of the discharge to its igniting power which shall be generally applicable to all mixtures.

10. *Preliminary Experiments.*

But before entering on an account of the main experiments one possibility which has been left unnoticed must receive attention. It has been tacitly assumed that the igniting power of a discharge is determined wholly by that of the individual sparks, of which it consists; but it is clearly possible that a mixture might be ignited by a rapid stream of similar sparks when a single spark of the same nature could not cause ignition. The experiments already described in § 4 provide evidence against such a supposition, for it will be remembered that it was found that if a discharge did not ignite the mixture when the first spark passed it would not ignite it, however long the discharge was continued. However, as the matter is very important, a preliminary series of experiments was undertaken to settle the matter.

One possible source of difficulty should be mentioned. It has been noted by all observers on ignition by the electric discharge that it is impossible to obtain perfectly consistent results. It is often found that a discharge which will ignite a mixture at one trial will fail to ignite it when the experiment is repeated in circumstances apparently exactly similar; the critical intensity for ignition has always to be taken as that which will explode the mixture in some definite proportion of trials.

Such inconsistency might make it hard to determine with certainty whether an increase in the number of similar sparks

passing in succession really increased the igniting power the discharge. For if we found that ignition occurred more often when two sparks were passed in rapid succession than when only one spark passed, we might be doubtful whether the increase in frequency was due to the passage of the first spark aiding ignition by the second, or simply to the fact that double the number of single sparks passed in a given number of trials, so that the chance of one of them causing ignition was increased. The doubt could only be settled by a somewhat elaborate investigation of the frequency with which discharges consisting of different numbers of successive sparks caused ignition ; such a statistical inquiry would be very laborious, and its results would always be open to some doubt.

Fortunately, however, it was not necessary to consider these complications, for no consistent increase whatever could be found in the frequency with which a given discharge caused ignition as the number of individual sparks passing in succession was increased. A single spark followed by no others caused ignition as frequently as two, three or four sparks passing in rapid succession. The arrangement described in § 5, where the number of sparks passing is limited by the quantity of electricity supplied, provided a convenient means for trying the experiment ; in order to vary the number of sparks which passed it was only necessary to vary the initial potential of the condenser C_0. The interval between successive sparks obtained by this method is also as short as can be readily obtained ; it was not measured directly, but was certainly less than the estimated maximum duration of a single spark and probably did not exceed 0·00001 sec.

It is permissible, then, to conclude that if a single spark will not explode a mixture, a succession of a small number of similar sparks in a very rapid succession will not explode it, and therefore that the igniting power of any discharge is simply that of the individual sparks of which it consists. Further, since the frequency of ignition does not increase at all with an increase of the number of sparks from one to four, it is clear that the condition of which the variation causes the inconsistency in successive trials must be such that it does not change in the small interval which separates successive individual sparks in the same discharge. But a limitation to the conclusion must be noted. It has already been explained that the individual sparks which follow the first are

not entirely similar to the first; they seem to carry a rather smaller quantity of electricity and probably pass at a lower spark potential. We shall see presently that the igniting power of a spark decreases rapidly with the spark potential; accordingly the fact that the mixture always explodes, if at all, at the first spark may only be due to the passage of the succeeding sparks at a lower spark potential. Since, however, this difference between the first and succeeding sparks is likely to be present in all forms of the discharge, the practical conclusion that the igniting power of the discharge is determined wholly by that of the first individual spark remains unaltered.

Again, it appears necessary to insist that in the application of the proposition the limitation to "a small number" of successive sparks be maintained. Cases were occasionally observed in which, though the mixture did not ignite at the first spark, it did ignite when the discharge had been continued for some seconds or even minutes and many thousands of sparks had passed. It is thought probable that such observations, which were very irregular, are to be attributed to the inconsistency which affects all the observations; the mixture exploded after a long time because in the interval since the first spark the variable condition which in part determines the ignition had changed; the mixture had simply passed into a more easily explodable condition.

After these preliminary remarks we can proceed to consider the main experiments.

11. *The Main Experiments: The Apparatus.*

Since the work was originally undertaken with a view to its application to petrol engines, it was thought desirable to make the main experiments on mixtures of petrol and air, although the exact conditions of the engine cylinder could not be reproduced, while the use of a liquid fuel introduced many manipulative difficulties. The chief of these difficulties arose in the filling of the explosion vessel with a mixture of known composition and from the necessity of maintaining it at a temperature of 50°C. in order to secure complete evaporation of the petrol.

It was expected from the outset that trouble would be found from the source of inconsistency already discussed; and every effort was therefore made to render the conditions of experiment capable of exact reproduction. The plan of

using as the explosion chamber some kind of engine cylinder, of which the filling and evacuation would be effected automatically, was rejected in favour of an apparatus in which the filling could be controlled more accurately, although much extra labour was involved. It may be said at once that the expected greater consistency hoped for was not realised, and that it would probably have been better to have adopted the less laborious method of operation.

A drawing of the explosion chamber used is given in Fig. 5, which needs little explanation. The spark gap is between steel

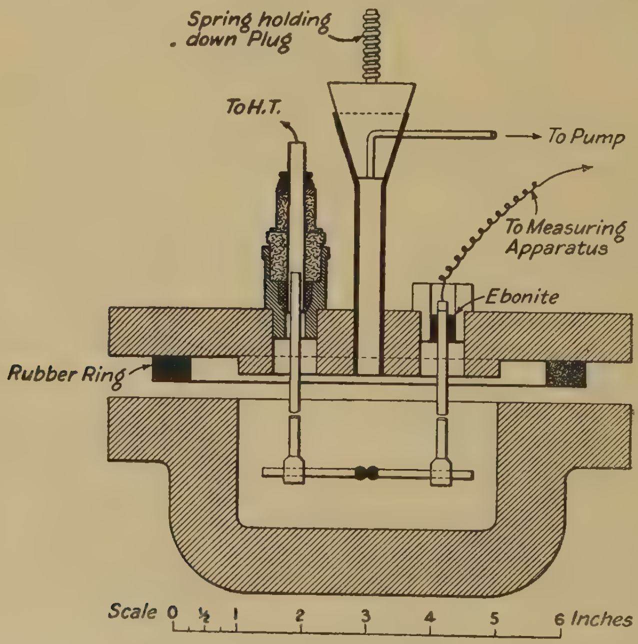


FIG. 5.—EXPLOSION CHAMBER.

balls $\frac{3}{16}$ inch in diameter, the distance of which is adjustable. The low potential ball, connected to the measuring apparatus, is carried on an insulated support made from a Lodge sparking plug with mica insulation; but, since it is important to keep the capacity to earth of the high potential electrode as small as possible, the support for the other terminal was specially constructed with this object in view.

The whole chamber was immersed in a water bath kept within 1° of 50°C., the water level being about ½ in. above the

upper surface of the plate which supports the electrodes. The tap, projecting above that level, was kept hot by means of a small flame.

The petrol was injected directly into the chamber by removing the plug of the tap and passing a graduated pipette into the tube connecting the tap and the chamber. The plug was then inserted and screwed down ; air was introduced along the tube by means of a motor-tyre foot-pump until the desired pressure was read on the gauge, which had been calibrated. The tap was then turned to isolate the contents of the chamber, which was then sufficiently air-tight not to lose more than 5 per cent. of its excess pressure in half an hour. The observations on the ignition of a single filling never occupied more than 5 minutes, so that the leak during this period was inappreciable.

For the purpose of stating the composition of the mixture in the chamber, the quantity p will be used ; p is the ratio of the weight of the petrol injected to the weight of the air which would occupy the chamber at the same temperature and total pressure ; p is therefore not equal to, but rather less than, the ratio of the weight of petrol to the weight of air in the mixture, for some part of the total pressure was due to the petrol vapour and not to the air. For an exact knowledge of the relation between p and the weight-ratio of petrol/air, information concerning the effective molecular weight of the petrol would be required ; such information is not sought, for the values of p given are used only to identify explosive mixtures of given composition and no use of the values is made for stating quantitative conclusions. The only assumption made in the course of reaching the conclusions to be given is that mixtures for which p is the same have the same chemical composition, whatever the total pressure of the mixture. This assumption is true if all the petrol is present in the state of vapour at all pressures ; and if, further, the effective molecular weight of the petrol does not vary greatly with the total pressure (or, in other words, that there is no very great variation in the departure from Henry's law). The best proof of the truth of the assumption, at least in the degree necessary for this work, is that the value of p for the most easily exploded mixture was actully found to be independent of the total pressure ; for it is probable that the chemical composition of the most easily exploded mixture is nearly independent of the pressure.

After the ignition experiments had been conducted and whether or no an explosion had occurred, the plug of the tap was removed again and a tube, connected to a Fleuss air-pump and calcium chloride vessel, inserted into the opening of the tube. The fit was tight enough for the pressure to be reduced to a few millimetres of mercury. The evacuation served to clean out the vessel and also to restore the insulation of the electrodes which had often been spoilt by the deposition of water. The cycle of filling and evacuating the vessel took $3\frac{1}{2}$ minutes, a fact which explains why the number of observations is so limited.

For the regulation of the discharge and the obtaining of a definite number of single sparks the method § 5 was employed.

A high resistance R (from 15 to 200 megohms) was inserted between the switch and the gap, in order to limit the discharging capacity to that on the low potential side of this resistance, as explained in § 5. The capacity of the gap with its connections to R was 16·0 mmf.; and this was the minimum capacity with which observations could be made. The capacity in parallel with the spark gap could be increased by adding condensers of known capacity; the values of these capacities were chosen so that the total capacity could be varied by steps of between 2 mmf. and 3 mmf. between the limits of 16 and 930 mmf. The smaller condensers added were made of rubber cable with an exterior coating of tinfoil, the larger of iron rods covered with micanite on which was wrapped a similar coating. The insulation resistance of the whole system of condensers and spark gap was never less than 10^{11} ohms and usually greater than 10^{12} ohms.

12. *Course of the Observations.*

In taking observations the spark gap was first fixed and the explosion chamber immersed in the water bath. A series of observations on explosions were then taken in which the total pressure of the gas in the chamber, measured by the pressure gauge, was constant, and the proportions of the mixture altered by varying the amount of petrol injected into the chamber. In each observation, after the chamber had been filled, a series of single sparks was sent through the mixture, while the capacity in parallel with the gap was increased in steps.

The inconsistency of consecutive trials was so great that it was unnecessary to increase C by the smaller steps possible

except at the lowest values. The following series of values of C was adopted in most of the work :—

16·0, 21·2, 24·4, 32·6, 46·8, 71·8, 121, 177, 225, 272, 346, 388, 426, 478, 542, 599, 701, 808, 916 mmf.

At each value of the capacity 10 trials were made, unless the mixture exploded before the 10 were completed. In most cases, of course, explosion occurred (if at all) at some trial between the first and the tenth. In order to give a numerical value for the critical capacity, the following rule was adopted : If the mixture had failed to explode in 10 trials with the capacity C_1 and exploded at the mth trial with the next larger capacity C_2, then the critical capacity was taken as equal to

$$\frac{(10-m)C_1+mC_2}{10}.$$

No theoretical justification whatever is claimed for exactly this procedure ; but it probably served as well as any other for fixing definitely a numerical value which could be used in the comparison of results.

When the ignition of a sufficient number of mixtures of different compositions had been investigated at one given value of the total pressure of the gas, a similar series was taken for each of several other values of that pressure. And when such series at different pressures had been completed, the length of the spark gap was changed and the whole procedure repeated.

The length of the spark gap was not measured directly, but readings were always taken, before and after a series of observations, of the spark potential of the gap in air (without admixture of petrol) at the pressure used in the series. When a petrol-air mixture is substituted for air, the spark potential decreases somewhat. A preliminary series of measurements was taken to find how the spark potential varied with the composition of the mixture ; these experiments were made with values of C and V so low that the mixture would not explode when the spark passed. It was found that the results were represented with sufficient accuracy for the purpose in view by the relation,

$$V=V_0(1-0{\cdot}6p),$$

where V_0 is the spark potential in air and V that in the mixture p at the same total pressure. Since p only varied

between 0·07 and 0·16 the total variation of spark potential due to changes in p was only some 5 per cent.

13. *Results Obtained with Petrol Mixtures.*

The complete record of the observations taken is given in Table II. At the head of each table, P is the total pressure of the gas in lb. per sq. in. (absolute); V is the spark potential of the gap in pure air at that pressure. In the following columns are given the values of p for the various mixtures

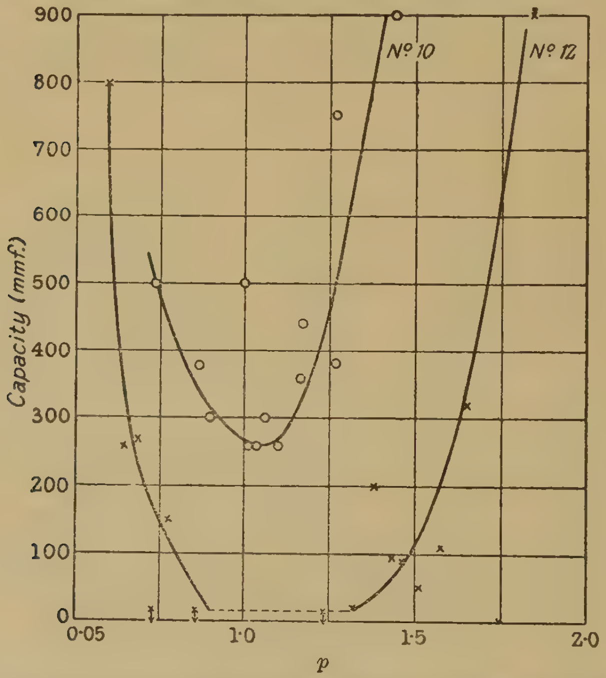


Fig. 6.—Critical Capacity and Composition for Petrol Mixtures.

used and the corresponding values of C, the critical capacity necessary to cause explosion.

In order to indicate the nature of the results obtained and their mutual consistency, two typical series (Nos. 10 and 12 of Table II.) are shown in Fig. 6, in which the critical capacity is plotted against the value of p.* It will be seen that the

* Where the critical capacity was greater than the greatest or less than the least capacity available experimentally, the corresponding point is placed at the greatest or least capacity and distinguished by an arrow pointing up or down.

general nature of the results is clearly indicated and accords with expectation ; the critical capacity has a marked minimum and increases rapidly for values of p either greater or less than that corresponding to the most explosive mixture. But it is obvious also that the observations are not sufficiently consistent for any accurate quantitative conclusions to be drawn from them. However, since some attempt had to be made to combine the observations further, the following procedure was adopted.

A curve was sketched through the experimental points of all diagrams such as Fig. 6 in the manner shown in those

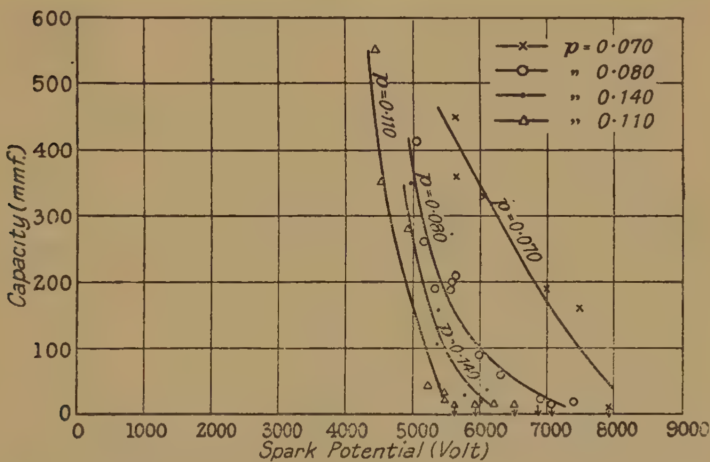


FIG. 7.—CRITICAL CAPACITY AND SPARK POTENTIAL (PETROL MIXTURES).

diagrams. From these curves values of C, the critical capacity, were taken off at various values of p. A comparison of the resulting set of values of C for various values of p, of the spark potential, and the pressure of the mixture seemed to indicate that C was a function of p and of the spark potential only, and did not depend on the pressure of the gas, except in so far as this pressure determines the spark potential; that is to say, if we keep the chemical composition of the mixture (i.e., p) constant and vary the pressure and the spark gap together in such a way that the spark potential is constant, then the value of C, the critical capacity, is constant. It is not certain, or even probable, that this proposition is generally true ; but it is not unlikely to be true over the comparatively

narrow range of spark lengths and pressures used in these observations. Since the accuracy of the observations was certainly not sufficient to prove it untrue, it was adopted as a necessary means to co-ordinating the results further.

Adopting this assumption we obtain, for several values of p, C as a function of the spark potential. The values so obtained are plotted for typical values of p in Fig. 7, in which the abscissæ are the spark potentials, the ordinates the critical capacities. The process of smoothing the curves in the

TABLE II.

1. $P=91{\cdot}3$, $V=6640$

p.	C.
0·092	90
0·114	22
0·126	40
0·118	16
0·121	16
0·123	16
0·126	16
0·131	50
0·136	30
0·143	50
0·151	55
0·160	90
0·079	60
0·092	22
0·082	70
0·101	16
0·087	22
0·081	22
0·078	60

2. $P=72{\cdot}3$, $V=5840$

p.	C.
0·117	30
0·136	40
0·146	90
0·165	900
0·096	60
0·084	110
0·074	320
0·051	900
0·106	55
0·128	100
0·119	45
0·129	60
0·140	100
0·154	900
0·123	70
0·110	33

3. $P=81{\cdot}4$, $V=5860$

p.	C.
0·125	23
0·150	220
0·130	16
0·136	30
0·094	60
0·085	170
0·064	500
0·076	250
0·088	120
0·096	20
0·108	20
0·112	16
0·120	16
0·131	20
0·139	27
0·144	60
0·156	100
0·167	900

4. $P=62{\cdot}7$, $V=4800$

p.	C.
0·110	300

5. $P=53{\cdot}1$, $V=4760$

p.	C.
0·110	550

6. $P=72{\cdot}3$, $V=7900$

p.	C.
0·064	130
0·124	16
0·143	30
0·152	45
0·166	90
0·186	170
0·216	900
0·052	230

7. $P=62{\cdot}7$, $V=7450$

p.	C.
0·080	16
0·090	16
0·140	16

8. $P=72{\cdot}3$, $V=6300$

p.	C.
0·142	16
0·166	160
0·092	16
0·080	180
0·063	900
0·072	90
0·083	90
0·095	16
0·154	90

9. $P=62{\cdot}7$, $V=5860$

p.	C.
0·120	110
0·098	130
0·110	20
0·084	130
0·092	130
0·100	50
0·114	20
0·129	16
0·146	260
0·138	900
0·125	30
0·133	60
0·152	400
0·095	60
0·078	70
0·082	260
0·074	260
0·069	420

10. $P=53{\cdot}1$, $V=5280$

p.	C.
0·144	900
0·116	440
0·103	260
0·110	260
0·126	750
0·100	500
0·116	300
0·101	260
0·116	360
0·126	380
0·090	300
0·086	380
0·074	500

11. $P=62{\cdot}7$, $V=5620$

p.	C.
0·110	16

12. $P=81{\cdot}4$, $V=7280$

p.	C.
0·151	50
0·143	95
0·132	20
0·146	90
0·124	16
0·138	200
0·158	110
0·184	900
0·165	320
0·086	16
0·072	16
0·064	260
0·060	800
0·068	270
0·077	150

13. $P=91{\cdot}3$, $V=7780$

p.	C.
0·069	250
0·077	16
0·073	16
0·066	290
0·165	110
0·158	300
0·148	90
0·143	16
0·148	270
0·141	90
0·128	20
0·135	16
0·168	650

14. $P=53{\cdot}1$, $V=8250$

p.	C.
0·07	16
0·09	16
0·13	16
0·14	16

15. $P=53{\cdot}1$, $V=5580$

p.	C.
0·120	150
0·097	16
0·108	16
0·134	150
0·128	200
0·116	90
0·090	16
0·078	250
0·071	580
0·087	55
0·143	900
0·133	200
0·118	540
0·107	30
0·098	16

17. $P=91{\cdot}3$, $V=5440$

p.	C.
0·124	21
0·151	900
0·135	300
0·130	60
0·115	170
0·184	16
0·072	700
0·076	280
0·078	250
0·084	250
0·089	400
0·094	220
0·110	60
0·103	16
0·096	16
0·118	22

preceding stage has clearly not been carried far enough, for the points are still scattered; but again they are sufficient to indicate the general nature of the relations. C decreases very rapidly as V increases, the rate of increase being greater the more easily explosive is the mixture. (Points nearer the origin indicate explosive conditions requiring for ignition smaller values of C or V). There is no indication whatever

of an approach to a critical minimum capacity common to all values of p or independent of the spark potential; there is nothing in the experiments to indicate that a capacity, however small, would not ignite the mixture if only the spark potential to which it is raised before discharge were sufficiently great.

The curves of Fig. 7 represent completely all the quantitative relations which can be deduced with any certainty from the observations. It would be absurd in view of the inaccuracy of the results to attempt to express the relation between C

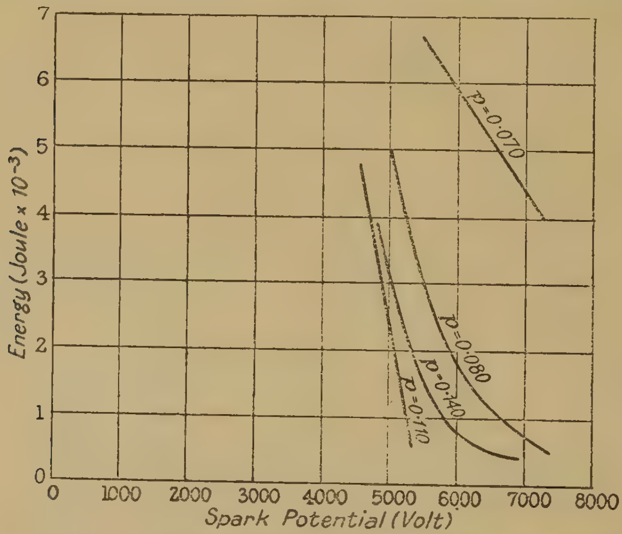


FIG. 8.—CRITICAL ENERGY AND SPARK POTENTIAL (PETROL MIXTURES).

and V as an analytical function or to base on their precise form any theoretical conclusions. But it is interesting to transform the curves slightly to bring out another point. When the quantity of electricity CV discharges across the gap, the electrical energy degraded is $\frac{1}{2}CV^2$ and this energy must also be supplied in order to cause ignition. In Fig. 8, deduced immediately from the curves in Fig. 7, this energy required for ignition is plotted against the spark potential. It will be seen again how very rapidly the energy decreases as the spark potential is increased; a change of 10 per cent. in the spark potential may involve an increase of 100 per cent. in the energy required for explosion.

One further matter may be mentioned. It was noted in §5 that Q, the quantity of electricity passing in the spark, was only found to be actually equal to CV, when C and V did not exceed a certain limit; when the limit was exceeded irregular measurements, sometimes of the wrong sign, were observed. The explanation which was offered would indicate that the limit is determined largely by the surroundings of the spark gap and that it does not represent any essential change in the nature of the spark. A series of observations was made to determine whether there was any sudden change in the igniting power of the spark in the region where the readings of Q became irregular; no indication of such a change could be found, but the inconsistency of the observations was such that small changes might easily have been overlooked.

14. *Experiments on Hydrogen-air Mixtures.*

The work of all previous investigators has shown that there is no very essential difference between the results obtained with mixtures of hydrogen and air and those with mixtures of hydro-carbons and air; the general relation between the "intensity" of the spark and the composition of the mixture is the same, the differences being merely numerical. Accordingly it was thought at this stage permissible to abandon petrol for a gas, which is so much easier to handle; and that such conclusions as were sought might be safely transferred from one fuel to the other. The remaining experiments were made with mixtures of hydrogen and air at atmospheric pressure and temperature. The mixture was contained in an explosion vessel made from a 3 inch shell through the walls of which the electrodes were supported by insulating bushes. One of the electrodes (which was always the kathode) was carried by a micrometer screw the nut of which was fixed in the bush, the junction between the two being made air-tight by rubber tubing. The screw could thus be turned from outside the vessel and the distance between the electrodes varied without interrupting the experiments. The method for controlling the discharge was unaltered.

The vessel was perfectly gas-tight and the mixture was often made by exhausting the vessel, filling to a known pressure with hydrogen and then filling up with air; but when a large number of observations were made on the same mixture a large volume of it was prepared in an external vessel. The composition of the mixture is denoted throughout

by r, which is the ratio by volume of hydrogen to the total mixture; it may be noted that the value of r corresponding to complete ignition of the oxygen in the air is 0·296. The pressure in different experiments varied between 761 mm. and 774 mm., the temperature between 17° and 21·5°C. No account is taken of these variations, but, of course, the variation in the experiments of any one series, taken at the same time, is much smaller.

15. *Stepped Ignition.*

In some preliminary experiments an attempt was made to discover the cause of the inconsistency of the measurements to which reference has been made previously. No success was attained, although much care was taken in the preparation and filling of the mixture. Moreover the inconsistency is as great when the composition of the mixture is such that it is near that of maximum explodability (so that small variations in composition should have very little effect) as when it is on the limits of explodability. It is certain, therefore, that variations in the chemical composition of the gas is not the cause of the inconsistency.

But in the course of these observations a curious fact was noted. In most of them the variation of the igniting power of the spark was effected by changing the distance between the electrodes (and so the spark potential), instead of by changing the capacity, as in the previous experiments. It was observed that, though at successive trials different values of the spark potential with a given capacity were required for ignition, these different values always tended to be grouped round two, or sometimes three, values; that is to say, sometimes a spark potential a was required and sometimes b, but never a spark potential clearly intermediate between a and b. The explanation which first suggested itself was a defect of the screw moving the electrodes, but it was eventually established, by actually measuring the spark potential in each case that the discontinuity in the spark potential required actually existed.

The experiments of Thornton were then remembered, which led him to speak of "stepped ignition." What he found was that, as the composition of the explosive mixture was varied continuously, the intensity of the spark required for ignition varied discontinuously; at certain stages a very slight variation in composition would produce a very large change in the

critical intensity, while variations in the composition between the limits at which these sudden changes occurred produced no change at all in the critical intensity. His conclusions have not been confirmed by other observers, but since here again an appearance of discontinuity (though of a rather different kind) was found, it was thought worth while to investigate the matter further.

Accordingly detailed experiments were made on the relation between the capacity and the spark potential necessary to

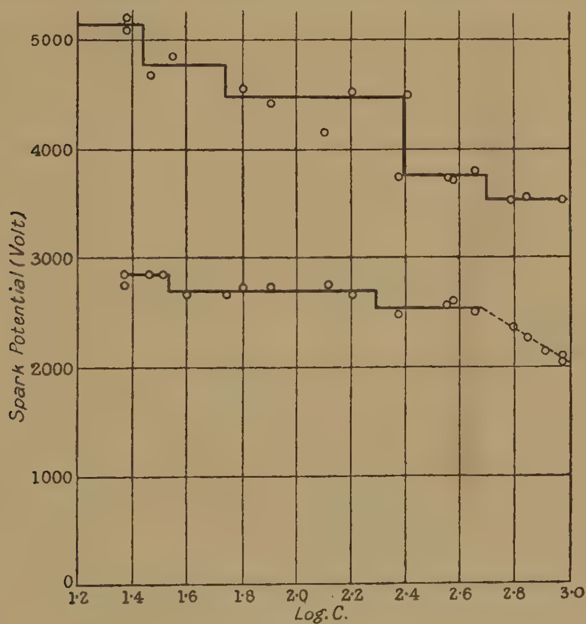


FIG. 9.

cause ignition, the mixture remaining the same throughout any one series of observations. The results thus correspond to those shown in Fig. 6; the only difference being that, while in Fig. 6 the relation was established indirectly through observations in which first the effect of varying the capacity was determined and second the effect of varying the spark potential. now the relation could be established directly because both the capacity and the spark potential could be easily varied.

The results seem to confirm the suspicion that the variation of the igniting power of the spark with the capacity or the

spark potential is discontinuous; two typical series of observations are given in Fig. 9, each series referring to a mixture of different composition. The spark potential necessary for ignition is plotted against the logarithm of the capacity in parallel with the gap. (The logarithm is chosen only to compress the scale of the diagram.) In the upper series, if not in the lower, the results are certainly represented better by a "stepped" line than by any continuous curve. The only point which lies notably off the stepped line occurs where a "step" is unusually large, and where, therefore, it may be suspected that an intermediate step has been omitted. Again, the ever present inconsistency (from which the two series of observations selected are unusually free) showed itself in the appearance of an observation on the wrong step rather than in a position between two steps.

Much more elaborate and detailed experiments than were possible in a research mainly directed to practical ends are necessary to settle the matter; but the results are distinctly confirmatory of Thornton's conclusions. Moreover, a reason why others have failed to repeat his results can be suggested.

If reference is made to Fig. 11 (which will be discussed in detail later) it will be seen that all the points lie on a smooth curve, and that there is no trace whatever of "steps" or discontinuity. Now the observations plotted there (or at least some of them) were made under precisely the same conditions as those of Fig. 9; some of the observations from one figure might be plotted on the other. The difference between the two figures is that while in Fig. 9, which shows discontinuities, the spark potential requisite for ignition is plotted against the capacity, in Fig. 11, it is plotted against the composition of the mixture. Now the general form of Fig. 6 shows that the igniting power of a spark is very much more sensitive to changes in composition than to the capacity discharging in the spark; that is to say, a given percentage change in composition involves a very much greater percentage change of critical capacity. Accordingly the effect of substituting as abscissa composition for capacity is much the same as diminishing very greatly the scale of abscissæ in Fig. 9, while the scale of ordinates is unchanged. But if the abscissæ were so compressed in Fig. 9, the discontinuities would become very much less noticeable than they are, and the curve would appear as smooth as that of Fig. 11.

Here, we believe, is the reason of the failure of Wheeler and others to observe "stepped ignition." All those who failed used the "induction" spark and varied the intensity by means of the primary current broken. Thornton, on the other hand, varied the capacity. A given percentage change in primary current changes the igniting power very much more than a given percentage change of capacity, and the failure to observe stepped ignition with the induction spark may be due simply to the difficulty of varying the primary current in sufficiently small steps.

On the other hand, the experiments, if they are reliable, show that the discontinuity does not lie in the chemical properties of the mixture, as Thornton suggested, but in the physical characteristics of the spark. Such a change of view would probably make the phenomenon easier to explain; but while its existence is still not quite certain it would be premature to suggest theories to account for it.

16. *The Influence of the Electrodes on Igniting Power.*

All the experiments hitherto described were made with the same electrodes for the spark gap, namely, steel balls $\frac{3}{16}$ in. in diameter. Preliminary experiments had shown that the capacity and spark potential required for ignition depended to some extent on the nature of the electrodes; systematic observations were now taken. In these the capacity in parallel with the gap was constant and equal to 23·8 mmf.; the igniting power of the discharge was varied by changing the distance of the electrodes and so the sparking potential. In order to multiply the observations and give generality to the results, the igniting power was always determined for a series of different mixtures.

In the first series of observations the anode was always a steel plate 1·8 cm. in diameter while the dimensions of the kathode, which was always of iron or steel, were changed progressively. Since the sparking distance never exceeded 0·13 cm. the anode may be regarded as an infinite plane. The following kathodes were used: (*a*) a sphere 1·0 cm. in diameter, (*b*) a rod 0·254 cm. in diameter with its end turned to a form approximately hemispherical, (*c*) a rod 0·098 cm. diameter, with its end similarly rounded, (*d*) a needle 0·050 cm. in diameter, with a sharp point.

The results of these observations are given in Fig. 10, curves 1, 2, 4, 6, in which the spark potential necessary to cause

ignition with the capacity of 23·8 mmf. in parallel with the gap is plotted against r which defines the composition of the mixture. (In order to avoid confusion, the experimental points are omitted from curves 2, 3, 4; they lie much nearer to the smooth curves than those for curves 1, 5, 6; the curves in Fig. 11 indicate the degree of consistency attained in respect of these curves).

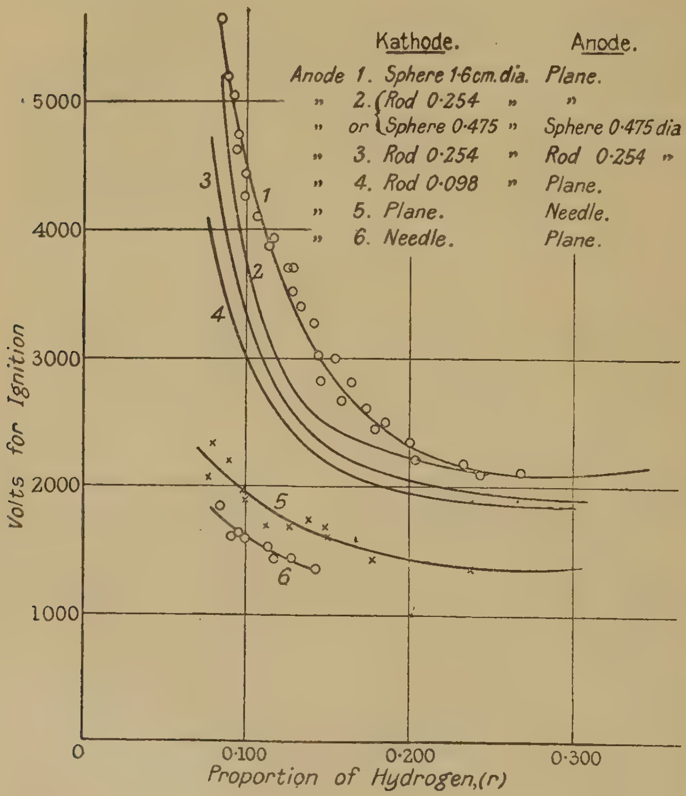


FIG. 10.

It will be seen that the spark potential required to ignite a mixture of given composition decreases notably as the radius of curvature of the sparking surface of the kathode is decreased, and that the decrease is more marked for the weaker, and less easily ignited mixtures.

Some further observations were made in which the plane surface was made the kathode and the electrodes of varying curvature was anode. In the case of the sphere of 1·6 cm. radius, no difference in igniting power due to the sign of the electrodes could be detected ; such a result is to be expected,

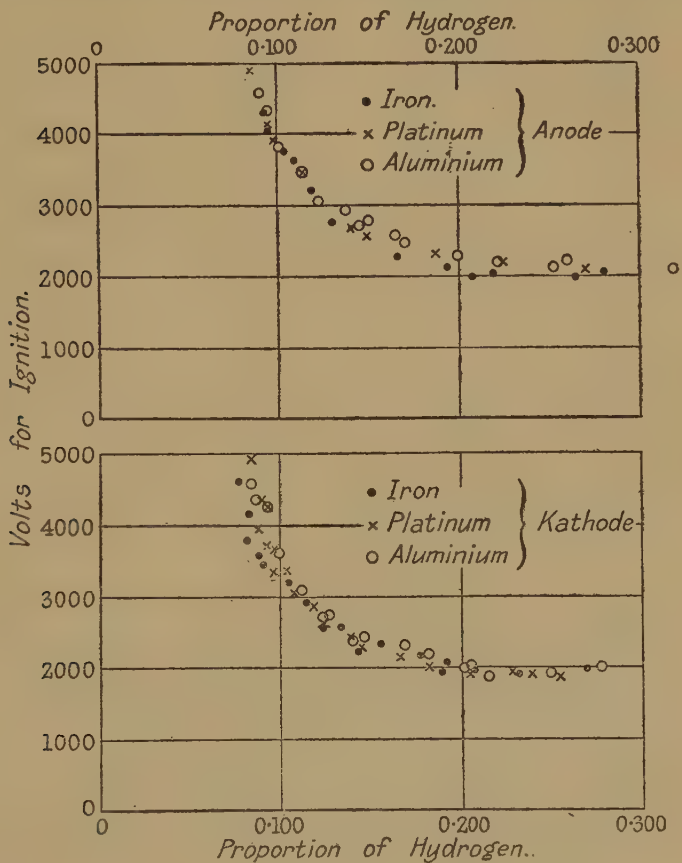


FIG. 11.

for, since the diameter of the sphere is large compared with the sparking distance, it can hardly differ materially from the plane which forms the other electrode. On the other hand, the reversal of the needle point and plane gap produces a marked change in igniting power ; the gap is decidedly less efficient in producing ignition when the point is positive (curve

5) than when it is negative (curve 6). The observations when the rod 0·254 cm. in diameter was the anode are not given in Fig. 10, but the observations of Fig. 11 to be described presently, indicate that again ignition is slightly less efficient when the finer electrode (the rod) is the anode than when it is the kathode. No experiments were made with the rod 0·098 cm. in diameter as anode.

Lastly, measurements were made when the two electrodes were similar: (*c*) when they were both spheres 0·475 cm. in diameter, (*d*) when they were both rods 0·254 cm. in diameter, (*e*) when they were both needles 0·050 cm, in diameter with sharp points. The electrodes (*c*) gave results indistinguishable from those of curve 2; electrodes (*d*) gave results which are plotted in curve 3. With electrodes (*e*) the results obtained were so irregular that they have not been plotted; but it did not appear that two points were any more efficient in producing ignition than the arrangement of point and plane. It may be pointed out that the observations on (*c*) and (*d*) fit in well with those when one electrode was a plane, if we regard the average curvature of the electrodes as determining the ease of ignition, for two spheres each 0·475 cm. in diameter give the same ignition as a rod 0·254 cm. in diameter and a plane, while two rods 0·254 cm. in diameter are intermediate between a rod 0·254 cm. and a rod 0·098 cm. in diameter, the other electrode being a plane.

Some experiments were also made to determine whether the material of which the electrodes are composed has any influence on the igniting power of a spark which passes between them at a given spark potential and with a given capacity in parallel. Since it has been shown that there is such a marked effect, due to the form of the electrodes, it is necessary to secure that electrodes of different composition have precisely the same shape*; accordingly, a plane was selected as the form of the

* Thornton has announced that the igniting power of "condenser sparks" is affected by the material of the electrodes. It is difficult to suggest any explanation of the order in which he places different materials in respect of ease of ignition; and his results have not been confirmed by those who use "induction sparks." Since he does not mention the possibility that the form of the electrodes, apart from the material, may have an influence and since the electrodes he used were all made from wire, it is very probable that the differences he found were due to variations in the form of his electrodes. On the other hand, an influence of the material of the electrodes on the igniting power of the "break spark" appears to be well-established; but since that "spark" is really an arc in which the discharge is doubtless conditioned by the material of the electrodes, there is no inconsistency between this result and the experiments now described.

electrode of variable composition. The other electrode was in all cases a steel rod 0·254 cm. in diameter.

Three metals were selected for examination, steel, platinum and aluminium; they were used both as anodes and as kathodes. The results of the measurements are shown in Fig. 11, where, as before, the spark potential necessary for ignition is plotted against the composition of the mixture. The upper half of the figure refers to the plane of variable composition as anode, the lower half to that plane as kathode.

The measurements do not establish any effect due to the composition of the electrodes; but on the other hand, they are not sufficient to establish certainly that there is not some small effect of this character. In each case the sparking potentials required with an aluminium electrode are consistently somewhat greater than those required with an iron electrode, platinum agrees more nearly with iron when it is a kathode and with aluminium when it is an anode. On the other hand, the differences between the results with any two different metals are not greater than those found between different series of observations, taken on different occasions, with the same metal. Again, the differences, such as they are, are more apparent with easily exploded mixtures when the variable electrode is the kathode and more apparent with mixtures difficult to explode when that electrode is the anode. Such discrepancies suggest that the differences found are due to accidental causes.

A few less systematic observations were also made with a view to discovering an effect of the material of the electrodes. First a brass rod was substituted for the iron rod of 0·254 cm. diameter without producing any apparent change in the igniting power. Second, there was substituted for the iron disc, 0·254 cm. thick, which formed the plane electrode a disc of the same dimensions of ebonite, covered with tinfoil. The other electrode in this case was the rod 0·098 cm. diameter, or the sphere of 1·6 cm. diameter. In neither case could any difference between the iron and the tinfoil disc be established.

The experiment just mentioned, in which an ebonite disc covered with tinfoil was used, was suggested by an idea that the thermal conductivity of the electrode might be important; an electrode of this nature would have a much lower thermal conductivity than any electrode of solid metal. But a simple observation made subsequently showed that such variations of thermal conductivity as it is possible to produce practically

are unlikely to have any effect. A small fragment of wax, melting at 48°C., attached to the surface of the tinfoil electrode, was found to be unmelted at the conclusion of the observations. If the surface of an electrode of such small conductivity does not reach a temperature of 50° during a single explosion, a further reduction of this temperature by increasing the thermal conductivity of the electrode would not be likely to have much effect. No influence of the thermal conductivity would be expected until the temperature reached by the electrode momentarily came near to the temperature required for ignition by a hot body; the conduction cf heat from the electrode would have to be reduced to an extent difficult to attain in practice before this condition was fulfilled.

17. *Discussion of Effect of Electrodes.*

It is clear then that the igniting power of a spark is not determined completely by the spark potential when the capacity in parallel is given; the form of the electrodes has a very considerable influence. The results suggest at first sight that the igniting power, for gaps of different form, may be more nearly determined by the sparking distance than by the sparking potential. The relations between the sparking potential and the sparking distance for the gaps used were investigated, and are employed in the following table, together with the values of the sparking potentials deduced from Fig. 10, to give the sparking distances which are necessary to cause ignition in two different mixtures and with various forms of electrodes.

Table III.

—	Sparking potential (volt).	Sparking distance (mm.).	Sparking potential (volt).	Sparking distance (mm.).
	r=0·100		r=0·250	
Sphere, 1·6 cm. diameter ...	4,580	1·04	2,330	0·48
Rod, 0·254 cm. diameter ...	3,800	0·87	2,330	0·48
Rod, 0·098 c,. diameter ...	3,070	0·89	2,060	0·46
Needle point (anode)	1,980	0·64	1,455	0·33
Needle point (kathode) ...	1,640	0·69	—	—

r is the ratio by volume of hydrogen to total mixture.

The figures show that though the variation in the sparking distance necessary for ignition is certainly less than that in the sparking potential, yet it undoubtedly exists. To obtain

a complete specification of the igniting power of a gap some much more complicated function of its geometrical form must be introduced.

The physical significance of the main conclusion attained is obvious. The less the radius of curvature of the surfaces between which the spark passes, the less will be the volume of gas in which before the passage of the spark, a given electric intensity prevails, and the less therefore, in all probability, the volume actually occupied by the spark when it does pass. If the same quantity of electricity is conveyed (as it will be if the capacity in parallel with the gap is the same) and the time of passage is not very different, the current density and the intensity of ionisation will be greater in the gap with the finer electrodes at the same sparking potential. The greater efficiency of the finer electrodes at the same spark potential is an indication that the intensity of ionisation produced by the spark, that is to say, the number of ions per c.c. existing at one time, is a very important factor in determining ignition.

18. *The "Explodability" of Hydrogen and Air Mixtures.*

Incidentally to these experiments some observations were made which deserve brief mention. Measurements of the pressure in the explosion chamber after the explosion had taken place indicated the proportion of the amount of hydrogen originally present which had been burnt in the explosion. It was found that in no case was all the hydrogen burnt as the result of a single explosion; above a certain limit of r the proportion burnt was independent of r, but below that value if decreased rapidly. (All the mixtures investigated were such that the proportion of hydrogen present was less than that required to consume all the oxygen in the air; if larger proportions were used, the ratio of hydrogen consumed would, of course, again diminish). In Fig. 12 the results of a systematic series of observations are given; the proportion of the hydrogen originally present which is consumed is plotted against r, which is the ratio by volume of hydrogen to total gas in the original mixture. A large number of other observations were taken incidentally to the main measurements, and it was found that the precise proportion of the hydrogen burnt at any given value of r varied considerably from time to time; in any one series the values were consistent, but repetition at another time would give rather

different results. Thus, for the maximum proportion burnt, when r lay between 0·11 and 0·29, values ranging between 0·942 and 0·888 were obtained on different occasions. But all series agreed in showing a very rapid fall of the proportion burnt when r fell below 0·11 and in all cases the least value of r which would give a mixture which would burn at all was very near to 0·075.

The failure of ignition to be complete is doubtless due to the cooling of the flame when it reaches the walls and it is to be expected that the exact ratios observed would depend

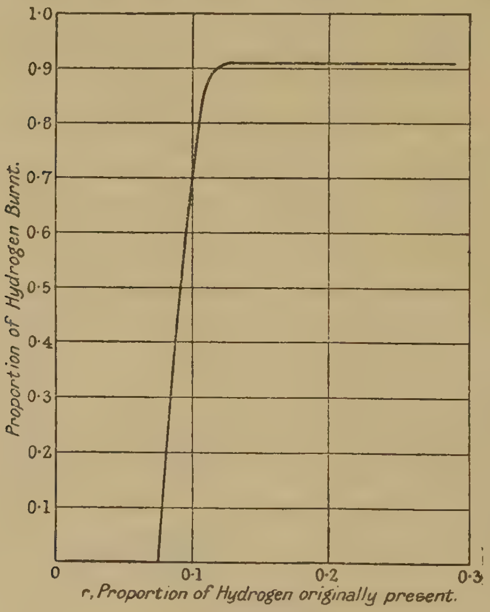


Fig. 12.

on the nature of the vessel and on the degree of turbulence. On the other hand, it is probable that the limits of r, between which a proportion of hydrogen is burnt which is greater than zero and less than the maximum for the particular vessel employed, is independent of the vessel and characteristic of the mixture only. It was shown by repeated observations that the proportion of hydrogen burnt was completely independent of the nature of the spark used to start the ignition; whether the spark potential and the capacity were such as only just to cause ignition at all or whether they were increased

to the greatest values practicable, the result of the explosion was precisely the same. Such a result is, of course, to be expected so long as the discharge is limited to the single spark which occupies a time short compared with that taken by the propagation of the flame throughout the mixture.

Observations of a similar nature have been recorded by many others who have investigated explosion. The distinction between a mixture that will "explode" and one which can only be "ignited" has already been established; it would appear that the range in the value of r throughout which the mixture is capable of ignition, but not of true explosion is that represented by the sloping part of the curve in Fig. 12. Richer mixtures than these are capable of propagating the flame throughout their volume and so are capable of true explosion; poorer mixtures cannot even start the flame and are incapable of ignition.

19. *Practical Applications.*

Any direct application of these results to the practical problems of engine ignition must involve an assumption concerning the relation between the form of discharge employed here and that given by the magneto. What this relation is has been suggested in § 8; it was there suggested that an induction spark would have the same igniting power as a capacity spark if in the two forms of spark the same quantity of electricity was conveyed between the same electrodes. Experiments to test this conclusion had been planned. but it is unlikely now that we shall carry them out; perhaps the problem will interest others.

However, another question of great practical importance was settled by experiment. Fig. 8 shows that in suitable conditions the energy required to explode such petrol mixtures as are employed in internal combustion engines is only a few thousandths of a joule, when the spark potential is as low as 4,000 volts, and is less than one-thousandth when the spark potential is 6,000 volts. The spark potential between the terminals of a sparking plug usually lies between these limits* and hence it seems that the energy required for ignition should not be greater than that just stated. Now the energy

* The temperature of the mixture in the engine cylinder, even when starting, is usually much higher than that of the mixture in these experiments. It is to be expected, therefore, that the energy required for ignition should be even less than that stated.

dissipated in the spark given by a magneto is usually about 0·1 joule, and in that given by a battery and coil ignition set about 0·03 joule. According to these experiments, these quantities are greatly in excess of what is required; the preference shown by some engineers for the magneto as against the coil on the grounds of its greater energy would seem to be groundless, and the efforts to increase still further the energy given by the magneto spark entirely useless.

But one important assumption is involved in the acceptance of this conclusion. We have only been concerned to determine what energy is required to produce an explosion at all; it is still possible that the explosion produced by a very feeble spark might develop less power than one produced by the expenditure of greater energy. The experiments described in § 10 indicate that there is no such difference between the effects of sparks of different energy, but the matter was one which ought to be settled definitely. In order that the practical bearing of the results might be as definite as possible, the experiments made with a view to settling the matter were carried out on an actual aeroplane engine working at full power as well as at lower powers; it was investigated whether the power developed when ignition was caused by a spark with only just sufficient energy to cause ignition at all was any different from the power developed when ignition was produced by the spark given by a magneto and dissipating a much greater energy.

The engine used was an 8-cylinder 70-H.P. Wolseley-Renault engine driving a dynamo brake by means of which the output could be measured; the magneto was of the B.T.-H. A 8 type. A switch was arranged so that the sparking plugs of the cylinders could be connected either to the magneto with the usual distributor or to a special igniting arrangement. This arrangement consisted of a transformer supplying direct current through a valve to a large condenser and thence through a resistance of 15 megohms, sufficient to isolate the spark gap in the sense of § 4, to a special distributor which connected the circuit to the sparking plugs in succession at the right moment for ignition. A micro-ammeter inserted between the high resistance and the distributor enabled the average current supplied for ignition to be determined. By adjusting the filament temperature of the valve the current supplied could be regulated so that in the interval between two successive sparks just enough electricity was supplied

to charge the spark gap up to its sparking potential; this condition was known to have been obtained when any further reduction of the current produced "missing" in the engine, showing that the spark failed to pass. From a knowledge of the current supplied, the sparking potential and the number of sparks per second the capacity discharging across the gap could be estimated. It was found to lie between 80 mmf. and 112 mmf.; it could be increased by placing a condenser in parallel with the sparking plugs, but owing to the necessary capacity of the somewhat complicated leads it could be reduced no further.

The experiments need not be described in detail, for the result of them was that the special igniting arrangement proved quite as efficient as the magneto with all petrol-air mixtures, from that which gave optimum power to those which were so ill-proportioned as to cause misfiring with the magneto. On switching over from the magneto to the special arrangement no decrease whatever in the power output of the engine was observed; when the engine was working at full power a decrease of 1 per cent. would certainly have been noticed. Now the magneto, according to tests made by its makers on another instrument of the same type, gave 0·09 joule per spark. In one experiment when the crank-shaft of the engine was revolving at 1,643 r.p.m., and the spark potential was 4,500 volts, regular ignition was obtained with a current of 33 micro-amperes; since there are four sparks for each revolution of the crank-shaft the energy per spark was

$$\frac{33 \times 4{,}500 \times 60 \times 10^{-6}}{1{,}643 \times 4} = 0{\cdot}0014 \text{ joule},$$

or about 1/65 of the energy given by the magneto. It is clear, therefore, that a very great reduction in the energy per spark given by the magneto would cause no loss of efficiency in ignition, and that any attempts to increase still further the energy of the spark are utterly mistaken.* Judged by modern standard, the compression in the engine on which these tests were made was low; the spark potential in a modern aeroplane engine with the same sparking gap would be about 6,000 volts, and the energy required for ignition would be

* It may be desirable to increase the whole energy output of a magneto in order to make it send a spark across a "leaky" plug. But such considerations are quite beyond our scope; we are considering only the energy of the spark when it passes.

still lower. It should be observed also that the figure given is not necessarily the least that is sufficient to cause proper ignition, for there was no evidence that, if the capacity in parallel with the gap could have been further decreased, ignition would have failed.

Summary.

The Paper is a continuation of that on p. 197.

(1) The previous part of the Paper showed that the "capacity" spark is the normal form of spark discharge, and that other apparently different forms of discharge differ from it only in consisting of a series of "capacity sparks," instead of only one. The present experiments were directed, therefore, to a more complete investigation of the igniting power of the capacity spark.

(2) It is shown that discharges which consist of a series of similar sparks have the same igniting power as a single spark of the same character; that is to say, that the ignition, if it occurs at all, occurs at the first spark.

(3), (4) The main experiments on the ignition of mixtures of petrol and air are described.

(5) The results show that the igniting power of a spark increases with both the capacity discharging and the spark potential, but varies much more rapidly with the latter factor. The energy required for ignition decreases rapidly as the spark potential increases, and there is no indication that, if the spark potential were sufficiently increased, the energy required for ignition might not be reduced greatly beyond the least measured in these experiments, namely about 0·0004 joule. The variation of the critical "intensity" of the spark (*i.e.*, either capacity or spark potential) is of the same nature as that found by other workers.

(6) The remaining experiments were made on hydrogen-air instead of petrol-air mixtures.

(7) The relation between the capacity and spark potential required for ignition is investigated more carefully. Indications are found of a phenomenon closely similar to the "stepped ignition" of Thornton, but the discontinuity seems to lie in the qualities of the discharge rather than in that of the mixture. It is suggested why others have failed to repeat Thornton's results.

(8) The influence of the electrodes on the igniting power is investigated. It is found that, with the same spark potential and the same capacity in parallel, the electrodes with the smaller radius of curvature give the greater igniting power. The materials of the electrode appears to have no effect on the igniting power.

(9) The results described in (8) are discussed.

(10) Some incidental measurements on the proportion of hydrogen burnt in the explosion are discussed. The distinction between mixtures which will explode and those which will only ignite appears clearly; the distinction is independent of the igniting spark.

(11) Some practical applications of the results are considered. Direct experiments on an aeroplane engine show that the energy required for satisfactory ignition is very much less than that in the spark given by an ordinary magneto or a battery and coil system.

DISCUSSION.

Dr. ECCLES said that, apart from the conclusions arrived at, there was a great deal to be learnt from the Paper. The ingenious arrangements in the diagram of apparatus included, for example, a thermionic valve used as a limiting device. It seemed worth while emphasising that these thermionic vacuum tubes were going to be invaluable as variable resistances in investigations in all branches of physics. The use of these tubes assisted in the present research in eliminating the oscillatory discharge of the condenser through the spark-gap. This, in fact, appeared to be a principal difference between the operation of the present apparatus and that of a magneto and sparking plug as ordinarily used, for it is incredible that the magneto discharge takes place without oscillations; the capacity and inductance of the leads themselves will ensure the presence of oscillations of high frequency, which may have a bearing upon the time taken to convert the initial electrical energy into heat energy in the spark-gap.

Turning to the question of the ignition of gaseous mixtures, what is required in a spark, whether electrical or from flint and steel, is that a quantity of heat should be given to a small mass of matter in such a way that the temperature reached is sufficient to inflame the mixture round it. This in its turn must develop heat by combustion so fast that the next layer of mixture shall reach the temperature of ignition. If the process proves to be "catching," the wave of inflammation will spread through the whole mass catastrophically. Evidently, for the success of this process, it is necessary that the heat shall be deposited more quickly than the thermal conductivity of the surrounding matter can carry it away, and therefore a given small quantity of heat is the more effective the smaller the time and the mass in which it is developed and the lower the thermal conductivity of the medium. From this point of view it is, therefore, extremely surprising to hear that magneto makers have aimed at increasing the energy per spark. The speaker would like to ask if the authors consider their experiments, which were likewise against the joules per spark point of view, accorded better with the simple aspect of the matter just outlined.

Mr. E. H. RAYNER said that from the diagram he thought the upper condenser could be made to give an oscillatory spark if required. In the case of plug sparking, the rate of rise of potential was as important as the actual voltage reached because of leakage.

Prof. LEES asked if the authors had formed any conclusion as to the mechanism by which the ignition is produced. The importance of the potential seemed to indicate that it was the speed of the electrons that mattered.

The AUTHORS, in reply to Dr. Eccles' remarks, communicated the following: The thermionic valve does not damp out the oscillations in the condenser which discharges through the gap; it only prevents certain other condensers discharging through the gap. There was distinct evidence that the discharge was oscillatory—at any rate, when the discharging condenser and the spark potential exceeded certain limits. The single sparks mentioned in the Paper doubtless often consisted of many oscillations; but these oscillations could not be separated, partly because their frequency was so great, partly because of the difficulties to which reference was made in stopping the discharge once it has started.

In the discharge produced by a magneto there are oscillations of two kinds. First, oscillations of the spark-gap circuit only, during which the terminals of the magneto act as free ends of the circuit; the frequency of these oscillations is probably (according to experiments by Mr. Albert Campbell and Mr. Dye) about 10^8; these oscillations are all contained in the single spark investigated in sec. 8, and are of the same nature as those which would occur if a suitable charged condenser were substituted for the magneto. Second, there are oscillations of the circuit which includes the spark-gap and the secondary of the magneto; the frequency of these oscillations is usually between 1,000 and 10,000. They give rise to the successive sparks, each probably involving oscillations of the first kind, which are seen when the discharge is viewed with a revolving mirror or the rotary spark-gap.

Dr. Eccles is doubtless correct in dividing the process of ignition into two stages, in the first of which combustion is initiated, in the second of which it is propagated throughout the mixture. In the second stage the process is undoubtedly thermal. Our experiments are not sufficient to determine finally whether the first stage is also thermal; but it is our opinion that it is not. We do not think that the development of a certain quantity of heat in the mixture is essential to the starting of ignition, but rather the development of a certain intensity of ionisation. This intensity of ionisation may be produced by thermal (*i.e.*, thermionic) means; it is probably so produced when ignition is started by a hot wire, and possibly, though not probably, when it is started by a flame; but it may be produced also by other means—*e.g.*, ionisation by collision or the action of ionising rays. The first of these means is probably employed in the spark discharge; the second when the ignition is started by the incidence of X-rays on a metal plate.

We do not quite understand why Dr. Eccles says that his view (*i.e.*, that the development of a small quantity of heat is essential to the starting of ignition) is inconsistent with the view that "joules per spark" are the determining factor in ignition. It appears to us that the two views are necessarily associated.

A Demonstration of Some Acoustic Experiments in Connection with Whistles and Flutes. By DR. R. DUNSTAN.

EXPERIMENTS were made with hollow spheres, cylinders and cones with holes of various sizes and in various positions. Bernouli's theorem, which gives the wave-length of the sound produced by a cylindrical pipe in terms of the length of the pipe and an end-correction depending on the diameter only, was shown to be quite inadequate for practical purposes, the pitch depending on many other factors, such as the wind pressure, the size and shape of the blow-hole, &c. Cylindrical flutes appear to require an end-correction which—within certain limits—is equal to D^2/d, where D is the diameter of the pipe and d the mean diameter of the mouth hole (which is often oval in shape). In the shortest flute experimented with, which was only $\frac{1}{2}$ in. long, Bernouli's theorem would give the wave-length as 2 inches, whereas it was actually 14 inches.

The conclusions drawn from the experiments are that in blowing across a hole in a hollow body a force existed on an elastic substance. The result is a "*spring back*," which produces an aerial throb, puff or pulsation. The frequency of the pulsation is determined by relations between the dimensions of the instrument, the size of the hole, the wind pressure, &c. Any resulting sound has its wave-length determined by the frequency and not *primarily* by the dimensions of the instrument, as in the usual text book treatment.

DISCUSSION.

Prof. BRAGG referred to Lord Rayleigh's work on open pipes and flasks and said that since the internal pressure of the air would vary with the size of the aperture it was easily conceivable that a difference of pitch would result when a change was made in the size of the aperture.

Mr. F. J. W. WHIPPLE outlined Lord Rayleigh's explanation of the long wave-length of the notes obtained from a hollow sphere.

Mr. T. SMITH asked if the variation in pitch with the position of the hole along the cylinder had been fitted to a formula—for instance, was the change in pitch proportional to the square of the distance of the hole from the centre of the cylinder?

Mr. NICOL spoke of the end correction, and stated that students often applied the correction to the *closed* end of pipes, which was incorrect.

Mr. F. E. SMITH said the end correction was useful in electrical problems, and gave an illustration of the variation of D^2/d in an electrical case.

The PRESIDENT asked if there was much variation in the ratio D/d, and said he thought the formula was important if it could be verified over a wide range. He referred to Lord Rayleigh's experiments on flasks as closely analogous to those of Dr. Dunstan.

In reply, Dr. DUNSTAN stated that the range of D/d was not very large in the instruments he had used.

A Demonstration of a New Polariser. *By* Mr. G. Brodsky.

In the course of experiments with polarisers built of piles of glass plates disadvantages due to bulkiness of the apparatus and loss of light had to be overcome.

The idea occurred to him to place the pile of plates between two prisms of the same glass in such a manner as to—

(*a*) Reduce the length of the polariser by one-half;

(*b*) Utilise the full aperture of the pile; and

(*c*) To get rid of *all* reflected light.

Results obtained with experimental prisms he showed were so good that they could be considered a very fair substitute for Nicol prisms of corresponding size, and the very small amount of light escaping through crossed prisms (which could be reduced further by additional plates) is for most purposes negligible.

There would be no difficulty in building such polarisers to any required size, as all the material consisted entirely of glass in unlimited quantities and at reasonable price, and it was hoped that this invention (Brit. Patent 121,906) would be used for many purposes.

Polarisers for directly transmitted light were hitherto very scarce and costly, so that many uses they could be put to remained undeveloped (such as stereoscopic kinematography). The new polariser could also be adapted to advantage in microscopes, saccharimeters, optical pyrometers, &c., and used for optical benches in the lecture room.

Experiments with piles of glass plates showed a very large discrepancy between the calculated and observed angle for best extinction. Whatever the glass used, and whatever the quality of the surface, this discrepancy came consistently to some 10 deg., whereas thin microscope cover plates were found to be useless.

There seemed to be still an interesting field for investigation as to the conditions affecting the surface of glass plates used in polarisers.

DISCUSSION.

Mr. T. Smith said it was difficult to discuss the merits of various forms of polariser, as many forms had been proposed and much was of a confidential character. He had been struck by the presence of two opposite tendencies in constructing such apparatus. In one group the optical portions of the apparatus were made large, and efforts were directed to increasing the size;

in the other group where the attainment of the same ultimate object was in view these parts were made as small as possible.

The PRESIDENT asked if it was not possible to get equally good results without prisms by reflection only. He thought prisms made the arrangement complicated.

Mr. BRODSKY, in reply, explained that he agreed with the remarks about reflection, but that his efforts were confined to transmission effects for a particular purpose.

A Demonstration of the Uses of Invisible Light in Warfare.
By Prof. R. W. Wood, *Johns Hopkins University.*

The first device shown was a signalling lamp consisting of a 6-volt electric lamp with a small curled-up filament, at the focus of a lens of about 3 in. diameter and 12 in. focus. This give a very narrow beam, only visible in the neighbourhood of the observation post to which the signals were directed. In order to direct the beam in the proper direction, an eyepiece was provided behind the filament. The instrument was thus converted into a telescope, of which the filament served as graticule. When directed so that the image of the observation post was covered by the filament, the lamp, when lit, threw a beam in the proper direction. In many circumstances the narrowness of the beam was sufficient to ensure secrecy; but sometimes it was not desirable to show any light whatever, and filters were employed to cut out the visible spectrum. By day a deep red filter, transmitting only the extreme red rays, was placed in front of the lamp. The light was invisible to an observer, unless he was provided with a similar red screen to cut out the daylight, in which case he could see enough to read signals at 6 miles. By night a screen was used which transmitted only the ultra-violet rays. The observing telescope was provided with a fluorescent screen in its focal plane. The range with this was also about 6 miles.

For naval convoy work lamps are required which radiate in all directions. Invisible lamps for this purpose were also designed. In these the radiator was a vertical Cooper-Hewitt mercury arc, surrounded by a chimney of the ultra-violet glass. This glass only transmits one of the mercury lines—viz. $\lambda=$ 3660 Ä.U., which is quite beyond the visible spectrum. Nevertheless, the lamp is visible at close quarters, appearing of a violet colour, due to fluorescence of the retina. The lens of the eye is also fluorescent. This gives rise to an apparent haze, known as the "lavender fog," which appears to fill the whole field of view. Natural teeth also fluoresce quite brilliantly, but false teeth appear black.

Reverting to the use of the lamps at sea, they are picked up by means of a receiver consisting of a condensing lens in the focal plane of which is a barium-platino-cyanide screen the full diameter of the tube. An eyepiece is mounted on a metal

strip across the end of the tube. When the fluorescent spot has once been found somewhere on the screen, it is readily brought to the central part and observed with the eyepiece. The range is about 4 miles, and the arrangement has proved invaluable for keeping the ships of a convoy together in their proper relative positions by night.

METROLOGY IN THE INDUSTRIES.

A meeting of the Physical Society of London was held at the Imperial College of Science, South Kensington, on Friday, March 28, 1919, Prof. C. H. Lees, President, in the Chair.

DISCUSSION ON "METROLOGY IN THE INDUSTRIES."

The President said: We have met this evening to discuss the question of "Metrology in the Industries." I think we all realise how very important the question is at a time like this, when the competition between the industries of this country and of foreign countries seems likely to become more severe than it has ever been in the past. I do not propose to occupy your time, but will call upon Sir Richard Glazebrook to open the discussion.

Sir R. T. Glazebrook, C.B., F.R.S., Director of the National Physical Laboratory: Mr. Chairman, ladies and gentlemen, I propose to speak briefly on this matter for various reasons, but mainly because I cannot in any way claim to be the originator of this discussion. Dr. P. E. Shaw, of Nottingham, who has taken great interest in metrology, suggested that there should be a meeting, and asked if I would contribute, and I said Yes, but without any intention of introducing the discussion. There are many here who can speak of the contact between metrology and the industries from more intimate knowledge than I can.

The connection between metrology and industry is, I suppose, really a very ancient one. The old Egyptians who built the pyramids must, if we are to trust what we are told by Egyptian antiquarians, have had a very considerable knowledge of metrology and of the method of applying it to building construction. So, too, must other ancient builders. Solomon, when he built the Temple, and Noah, when he built the first great ship, must clearly have used measuring apparatus of some kind, and although I do not want to go into the early history of metrology, such little knowledge as I have of it shows me that the early history of weights and measures is really a very interesting study. But I take it that in these early days, although people worked to considerable accuracy, if one may judge from what we hear and read about the Pyramids, still

the limits of accuracy were very wide. The application of metrology to industry—and by industry I rather mean the work of the mechanical engineer—to any high degree of accuracy is much more recent. I suppose we owe that application chiefly to Sir Joseph Whitworth. Sir Joseph Whitworth's surface plates and his measuring machine and gauges all were examples of the way in which the science of accurate measurement was applicable to industry, and the manner in which he succeeded in applying that science showed itself in the great accuracy and high value of the products of his firm, though, if one speaks of the accuracy of the applications of metrology, I am inclined to think that possibly the surveyors may claim to have reached even a higher pitch of accuracy than he did. If we look up the records of the piercing of the Simplon Tunnel, and consider how nearly the two lines, from the Italian and from the Swiss side, met in the centre, we shall understand that metrology as applied to engineering work of that kind and to surveying work has reached a very high position indeed.

Now, the value of Sir Joseph Whitworth's work was this, he taught people to make their delicate measurements of length with great accuracy, and he introduced—I am not sure that he was the first to introduce, but at any rate he extended its application—the use of gauges for accurate mechanical work. I think I am right in saying that the gauges he used were mostly of the nature of reference gauges. Some means of measurement were employed to compare the results of the work with the accurate gauges. The next step—and I do not know to whom that next step was due—was the introduction of the method of limit gauges. Perhaps some of the manufacturers here present could tell us rather more about its introduction. Instead of instructing a man to turn or grind a shaft 6 in. in diameter with a certain accuracy—say, of 5/1000 in.—and giving him a micrometer or some measuring machine to show him how near he gets to it, you give him two gauges, one 6 in. in width, the other, say, 5/1000 in. less, and all the workman has to do is to grind his work until the larger gauge will go over it, making sure that he has not ground it so far that the smaller gauge will go over. Thus, you have reduced the operation of gauging practically to a mechanical affair, and you do not want in that case anything like the same skill or the same knowledge on the part of the workman as you clearly do if he has no limit gauges to work to, but only some standard piece with which he compares his work by means of some method of measurement.

The first point then I want to make is this, that if you are going to produce work in quantity—if you are going to do repetition work of any kind to a large extent—you must make use of this system of limit gauging. Again, suppose you are only dealing with work produced in one and the same shop. It is not then necessary that the limit gauges should be accurate to any one standard dimension; provided the whole system of gauges you are using is consistent within itself, your work will be interchangeable work. It is not necessary, then, so long as you are concerned with work coming from a single shop, to be very particular as to the exact unit of length to which the gauges conform. But now suppose you go further, and want to make sure that the portions of work you are producing in a large number of different shops will be interchangeable, then it is necessary that not only the gauges in each shop should be consistent amongst themselves, but that all those gauges in every shop should be referred to some definite standard; that gave rise to the necessity for accurate gauges made in large quantity and identical or practically so with each other. So far as I am concerned, my own attention was first called to this particular point—the difficulty of getting interchangeable work from various shops—by some events which took place shortly after the beginning of the Boer War. It was found that the ammunition, especially the breech screws and other parts of guns, was not interchangeable when it arrived at the front, and a committee was appointed by the War Office, as a result of which a standard lathe was made, which is now at the National Physical Laboratory, in order to assist in the production of really correct leading screws for the various shops doing Government work throughout the country. That led up to the establishment of the Engineering Standards Committee on gauges, of which Sir Frederick Donaldson became chairman, and in the formation of which Colonel Crompton and other gentlemen played a prominent part; that committee set to work to consider the problem of producing accurate gauges and of defining the limits and tolerances that were required for interchangeable work. At first we had very little knowledge indeed as to the amount of these tolerances, or as to the value of the limits suitable for work. Then our ignorance on that score was relieved by a large piece of work undertaken by Mr. Attwell at the National Physical Laboratory. He spent some considerable time measuring up work of various kinds at a large number of shops throughout

the country, and specifying the actual tolerances and allowances that existed in practice, and from that the Committee deduced certain rules as to tolerances and allowances; so that by 1914 something had been done as to determining the tolerances allowable on gauges, and a certain number of firms had introduced the principle of limit gauge work, but, as a general rule, one may say that principle was not used generally in the shops of the country. Then came the cry in 1915 for a large supply of munitions and for the manufacture of munitions on a great scale, parts being made at various shops and at various institutions, all of which must be strictly interchangeable. The first scheme that was developed was one whereby the contractors made their own gauges, and, as any of you who have realised the difficulty of gauge making will appreciate, that led only to worse confusion. Few firms then were in a position to make gauges, and the few firms that were in that position were almost immediately swamped by the amount of work they were asked to do. Besides, there were other difficulties. It is true that drawings existed for the gauges, and firms could be told to make their gauges according to these drawings. But the real standard was not the drawing or the figures—the dimensions—indicated on the drawing; in too many cases it was a standard set of gauges in some Government department, and how near those gauges came to their nominal sizes was not known, with the result that the work first made was far from interchangeable, and difficulties arose. Sir Henry Fowler then took the matter up, he arranged with Mr. Ryan as to the supply of gauges and asked us at the National Physical Laboratory if we could do anything to facilitate the test. Just let me remind you of one step which was taken at once. By going carefully into the question of gauging of screws—by applying the simple principles of metrology to screw gauging—it was clear that you could at once halve the number of screw gauges which were needed for the work which was being put in hand. Half the screw gauges required originally were absolutely useless, and Sir Henry Fowler was able to strike them off at once. Of course, at first there were great difficulties. Gauges came in in considerable numbers, and when we began to test screw gauges the number of rejections was enormous—something like 75 to 80 per cent. At the end of two years' work, the figures went almost exactly the other way, and 75 to 80 per cent. of the gauges were passed. We had to face from time to time angry committees, who were

out to " win the war " with the least possible delay, and who found it difficult to appreciate that a few thousandths of an inch in a gauge made a difference. But many of those who were at first inclined to think the accuracy required and much that was being done quite unnecessary remained to bless after all. The reason for this was that through the war this interchangeable work proved itself absolutely necessary.

Now, I want to make as my second point, the point that if we are to maintain our position in peace, interchangeable work of this kind in engineering manufacture is equally absolutely necessary. If that is the case, parts of machines must be made to be interchangeable, so that we may have manufacture in quantity. Sub-division of labour is essential. Much is due to those who have produced this result during war-time, the persons who at the Laboratory and elsewhere studied the various problems and devised the methods of testing and the machines for testing, but far more, I think, to those who throughout the country threw themselves so heartily into the bringing to real practical effect the lessons that were taught by applying metrology to this great industry.

I need not go into the special difficulties of the measurements; I think they are well known. Much has yet to be done if we are to keep ahead. And if I may spend a few moments further on that point, I would say that in the first place I think it absolutely necessary that we at the Laboratory should improve our own appliances and our own methods of working to the very utmost. Then we must secure, I feel sure, that research should accompany all the testing work and examination work which is done in connection with gauges, as, indeed, with everything else. Improved methods of investigation grow out of the examination of the work that is being done; and important work follows from those methods of examination. It is necessary that testing work and research work in this as in other matters should go hand in hand. Any scheme which contemplates a separation between these two branches of work is to my mind, doomed to failure. But while that is so, and while I think it is necessary that there should be one authority looked to in the country to set up and maintain the standards, it is desirable also that there should be at various places throughout the country local institutions and organisations for testing and issuing tested gauges. This, I think, is desirable from many points of view. It is one way of interesting the manufacturers locally, and making them

acquainted with the advantages and with the necessity of accurate gauge work, and it is also a means of educating the persons who are going into industry, and who will help to carry on the work of manufacture by limit gauges through the instruction and teaching that they have received in such institutions. All such institutions must be closely linked up together, and every effort should be taken to secure that there is no setting up of various standards for various parts. We shall hear more on this subject from Dr. Shaw. We shall hear from him an interesting account of what he has been able to do with regard to education in Nottingham.

I trust I have indicated briefly some of the reasons which lead me to think that the applications and teaching of metrology to engineering manufacture are of the very greatest importance, that while we have done in the past few years a considerable amount of work there is yet much to be done, and for that work we need the co-operation of the manufacturers in the country and of those who realise that it is only by research of a somewhat advanced character that the progress we are looking for can be maintained.

Mr. Wm. Taylor (of Messrs. Taylor, Taylor & Hobson, of Leicester) said: We have all listened with great interest to the excellent sketch which Sir Richard Glazebrook has given us of the progress of metrology during the war. I have no doubt that as a result of the war this country and other countries will realise great advancement in this subject.

We are too apt to think, however, that all this progress has been made during the war, and to overlook a great deal of work which was done before the war. I believe that interchangeability in manufacture has existed in the watch industry, especially in Switzerland, for a century or even centuries. In the scattered farmhouses of Switzerland it has been the custom to make the parts of watches interchangeably to gauges—the jewels, the pivots and so on—and the parts have been brought down into the valleys for assembling. And in watchmaking the tolerances are very small—of the order of 1/10000 in. One reason why our own country has been behind some others—for it has been behind—in interchangeable manufacture is because we have not had here any flourishing manufacture of clocks and watches, which is a training ground for highly skilled mechanics accustomed to precise work. In America interchangeable manufacture, as we understand it to-day, together with the use of limit gauges, was first introduced, we are told, by Colonel Colt during the Civil War. From Colonel Colt's works it spread through concerns now well-known in the Eastern States, and long before it was generally understood in this country, interchangeable manufacture was being fairly widely practised in America. Nevertheless, it was practised in this country. I believe I am right in saying that when the watchmaking industry of Lancashire was a flouri hing industry, the parts were made as in Switzerland, and were interchange-

able. Many other manufacturers in this country have been carrying on interchangeable manufacture. The manufactures of sewing machines, bicycles and of ball-bearings are well-known examples, and, if I may mention my own firm's work, it is about thirty years since we first established the manufacture of the turned and threaded parts of photographic lens mountings within a tolerance of 0·001 in. interchangeably, and since I first made for measuring the Whitworth screw thread trigonometrically, the notched bar and the needles and prisms which have been so useful during this war.

Some beautiful work has been done by our makers of printing types, the widths of the faces of which have been brought within something like a tolerance of 1/10000 in. They have used microscopes, but have become so expert that it was said of one great maker of printing types, Sir Henry Stephenson, that with his unaided eye he could look at a type face, and say whether it was 1/10000 in. too large or too small.

We must not blame our manufacturers for not having been so advanced as we think they should have been. One disability was this: The Americans have the advantage of being specialists in manufacture, and they have a large market of their own, which is protected; we, on the other hand, are not to the same extent specialists, we have a relatively small market, and not even the unrestricted use of that. It is exceedingly difficult to restart the clock and watch industries in this country, because we are up against the competition of huge businesses established elsewhere, in one alone of which they are making from three to five thousand watches in a day. That is one reason why we have been backward in this country; we have not had large markets, and we have not specialised sufficiently. Perhaps, through greater specialisation and the economy which will accompany specialisation our manufacturers will find their markets in spite of handicaps.

I noticed in reading the advance proof of Mr. Shaw's Paper, which will be read presently, that he made mention of the need of teaching in our schools. I do sincerely hope that our engineering schools will begin to get busy in a new direction. Far too long they have been busy teaching the science of design and neglecting the science of the workshop. We have to dig out the science which should govern our workshop practice. It has not been brought together and put in form for teaching. I hope, for example, that somebody will write a treatise on the principles of gauging for interchangeable manufacture, and that succeeding speakers in this discussion will, among other things, tell us something more about the requirements of accurate gauging. It is important that our manufacturers should have measuring apparatus of the highest order. They must have good standards of length. They need to know whether these standards should be line measures, which are not subject to wear, but need to be translated into end measures by means of microscopes, or whether they should be end measures. If they are to be end measures, then we probably ought to develop in this country the manufacture of standards of the Swedish type, so that these may be available to everybody at very low prices. Nothing would do more to further the advancement of accurate measurement among engineers than the provision of cheap and accurate standards of this kind. We hear that Mr. Sears is developing methods of making them, and hope that he will show how they can be supplied at the cost of a few pence each.

Sir Henry Fowler, Ministry of Munitions, and Chief Mechanical Engineer, Midland Railway, said: The last speaker said that he came here with one object. If I have an object it is that I would like to utter an appreciation of the work done by those who have paid attention to metrology, and more especially to the wonderful work of the National Physical Laboratory, thanks to the guidance of Sir Richard Glazebrook. I am perfectly certain that the country does not know or appreciate the debt it owes to him. I was fortunate in having control of what I suppose has been the largest production job that the country has undertaken. We turned out somewhere about 200 million shells, and with that was associated the cartridge case, the fuse, friction tube, &c. The operations on a certain fuse were something like 600. It was a production job—a repetition job—of some size. Before I joined the Ministry of Munitions in 1915 I had had some experience of the difficulties which arose, not only with gauges, but in the measurement of gauges. We were, as I said, very fortunate indeed in having the National Physcial Laboratory to depend on, and I say "depend on" advisedly, because, outside those of the N.P.L., what appliances there were on which any great reliance could be placed were not, at all events, generally known. The inefficiency of the appliances which existed in some places, and which could not be relied on for giving consistent results, was the cause of very considerable trouble, and one which tended to discredit metrology. Now, in the early days I heard a good many things said against the National Physical Laboratory, but there was one thing I never heard. I never heard it suggested that any gauge which went to the National Physical Laboratory twice ever had two different reports made on it, and that is the whole matter with regard to the final opinion which came to be held as to the excellent work of the National Physical Laboratory. People grumbled at its very accuracy, but they appreciated very soon that its measurements were absolutely reliable, and they found that if the Laboratory decided that a gauge was out in a particular direction, that was the direction in which they had to work in order to get it right. Mr. Ryan, of whose work as Director of Gauges, one cannot speak too highly, will speak later and more authoritatively than I can do, and will tell you, perhaps, the difficulties which had to be overcome with regard to the screw gauges, and the way in which they were dealt with. There is one lesson which should not be lost. The lesson of the Boer War was not taken to heart, and when the great war broke out you could almost count on your fingers the number of perfect lead screws which could be used without any adjustment for the production of screw gauges. There is one thing which was always a source of worry to us, more particularly with regard to gauges, and that was a taper screw thread called the "G.S." (general service) plug. It does not taper in any well defined way, whilst it has not an even number of threads per inch. The only way in which we could think that thread was made was that it was cut when both the lathe screw and the bed were badly worn, and that the product was made the standard. That was one of the things we had to deal with. We had to try and get gauges which should meet these extraordinary conditions. We have been satisfied with regard to the screw gauges for ammunition, and although, perhaps, I am dealing more with design than with metrology, the necessity of standardisation of the various screws was

fairly well impressed on everybody. Mr. Ryan was getting perfectly happy on ordinary munitions of war until the aeronautical engine question came along, and then you had every kind of pitch and thread that had ever been thought of. The fact that, practically speaking, no question was ever raised with regard to the lack of interchangeability is, I think, the very highest testimonial which can be paid to the work of Sir Richard Glazebrook and Mr. Ryan. Mr. Taylor has said that interchangeability was in practice in this country before the war, and one respect in which this was so was the Renold chain. The fact remains that, undoubtedly with regard to the country as a whole, it did not appreciate the advantage of limit gauges. Now that we have gone to all this trouble to get interchangeability one does not want the work to be lost. It is not only a question of measurement, but of keeping the measurements right. I remember somewhere about fifteen years ago going to a motor-car works in Berlin—and the motor-car is another instance in which limits have been used for a considerable time—where the works were all sectioned up with expanded metal and a gauging system controlled each section. I was intensely pleased to see such care taken. I had a deal with certain cars made in this factory over in this country, and the very first time we bought spares we found they would not fit. It all goes to show that we may do things as carefully as possible, but we have got to bring this question down to our everyday concerns, and there is a very great deal of education still wanted in this direction. The country has had a wonderful lesson in experience, but it has not graduated yet. And there will be a tendency to go back again to the old ways unless we go on hammering at the subject. We can only produce economically if we can produce in quantity, and only produce in quantity if we go in for interchangeability, and we can only have interchangeability if this science of metrology goes ahead.

The Need of Metrology in the Universities. *By* P. E. SHAW, *B.A., D.Sc., University College, Nottingham.*

COMMUNICATED BEFORE THE MEETING.

Of all experimental sciences physics is *the* one which deals with measurements, for in no other science is there such variety, scope and delicacy of measurement performed. Metrology, the science of measuring, may be taken to embrace fine measurement of all physical factors for which a unit is definable. Its scope, however, as at present practised at the National Physical Laboratory, includes length, mass, time and simple derivatives of these, such as area, volume, angle, velocity, pressure. It may be desirable, however, before long to bring thermal, optical, electrical and other factors within the purview of the science. This subject is an applied science, independent alike of physics, of which it may be considered a derivative, and of engineering, for the purposes of which it is at present principally used.

Metrology, as we now know it, is almost synonymous with the length measurement of solid bodies, so greatly does the work performed in the metrological laboratory in this section exceed that done in all other sections put together. This state of affairs is due to the efforts of engineers, who from about 1840, when Sir J. Whitworth began his

pioneer work, have developed accurate methods of measuring over-all, under-all and other lengths in their machine parts.

Metrology made its first appearance as a separate organised science in this country about 1903, when testing work was commenced at the National Physical Laboratory. This Institution has ever pursued a generous policy of helpfulness to all comers, and under its fostering influences this new science grew rapidly in significance till 1914, when, with the stimulus of war, it at once developed into great importance. Standardisation was adopted by the nation as the only possible basis on which war munitions could be produced in the quantity and of the accuracy necessary for victory. Thus, as the essence of standardisation is accurate measurement, metrology at once took its place as an important component of the war machine : a science of great practical importance.

Although this subject has come to the engineering craft of this country as a by-product of the war, it is now permanently established, essential alike for the uses of industry and pure science.

The engineering trade is likely to extend greatly the principle of standardisation of output, as well as to adopt the standardisation schemes of the Engineering Standards Committee. For these and other purposes this trade will feel the necessity for more and more metrology.

In 1916 I undertook metrological work for a great shell factory. This led to the starting in University College, Nottingham, of a department of metrology. This was, I believe, a new idea, which might be adopted with advantage in universities and colleges—at least in those situated in manufacturing centres. The present time of transition seems opportune for a discussion of the subject. The work done in a metrology department in a university would in some respects be unique in character. It may be comprehended under three headings—Teaching, Testing and Research.

Teaching.—In organising a course of instruction in this subject one would make it chiefly, but not wholly, practical, and would only attempt it, as regards day students, in the cases of those taking final work in engineering and physics. Engineers should pass from the college to the works with a knowledge of the best modern practice as to the use of precision measuring tools. At present probably not one engineering student in a dozen on leaving college understands the rudiments of the principle of limit gauges or can claim to have had a systematic course in, for instance, the use of a measuring machine or of screw-measuring devices.

The physics student also would acquire some acquaintance of this subject, greatly to his advantage. It would add something useful to his technical equipment to know a really good measuring tool when he sees it ; to know something of its manufacture, how it should be used, and of what order of accuracy it is susceptible. At present we put before him, in the physics laboratory, apparatus often necessarily rough, and his results rarely attain an accuracy of 1 in 1,000. In laboratory teaching one has too often to warn the student that his present object is to acquire knowledge of physical principles and experimental methods, and that his numerical results will often be poor. The establishment of a department devoted to accuracy of measurement would inevitably

raise the standard of the apparatus in the teaching laboratory. We cannot fail to damp in some degree the enthusiasm of the inexperienced student by having inefficient instruments for his use.

There is one industry—instrument making—in which physicists are specially interested. This craft would certainly benefit if a knowledge of metrology were more widely disseminated. We want to have more instruments made in this country. In the past we have often had to send abroad for good and reasonably cheap apparatus of all kinds; and later, when repairs are required, we not infrequently send the apparatus back to the makers, because we lack their special facilities. There will soon be a great demand for apparatus to meet the coming activity in teaching and research, and in the industrial developments of physics and other sciences. We shall need more skilled instrument makers, and definite instruction in metrology would certainly be conducive to this end.

Testing.—The metrology department would have standards, such as those of length, mass and time, verified at the National Physical Laboratory. From these as a foundation the various measuring instruments in the university could be calibrated to a known order of accuracy. In like manner we could check instruments as they arrive, whether from our own workshop or from the instrument makers; and we could forestall trouble from defective apparatus whether destined for laboratory use or for research. It is bad business in buying measuring tackle to accept it at its face value. In many cases, given suitable and properly installed methods in a metrological laboratory, it would be easy to check the accuracy—*i.e.*, the money value—of the instruments on arrival.

If this testing system were organised we could give students excellent practice in the technique of instrument testing; but organisation and system are necessary. At present, in the absence of a metrology laboratory, the busy demonstrator can spare little time from routine work and his own research for the necessary testing of laboratory apparatus. The result is that the checking of values, if ever done, is sporadic, and on these rare occasions it is done laboriously and inefficiently; for good testing work requires technical skill attained after careful study of methods and preparation of appliances.

To take one or two instances. A resistance box may go wrong. Its polished top looks imposing, engraved white on black; but after years of use, and abuse, it loses accuracy; it may even be partly burnt out. And then the engraving should be of the nature of an epitaph: "Below this slab lie the charred remains of a once useful instrument."

Again, I once had a Whitworth measuring machine lent me as a favour. It was in use, and was considered to be in order; but, on finding the action very hard, I took out the micrometer screw, and actually found parts of the thread missing and other parts rusty. So far had a well-made tool fallen by ill-usage and neglect.

Of course, the metrology department would check apparatus belonging to various other college departments—lengths for the engineers, masses for the chemists, and so on. For the physics department much could be done in tests on length, mass, time and other units.

In a provincial city this department might be of great service to engineers in testing their standards, micrometers, check-bars, screws

and gauges generally. Standardisation is likely now to be greatly developed by engineering firms. This system breaks down and becomes more or less a failure unless precision is practised throughout in tool room, workshop and inspection room.

Industries, other than engineering, might avail themselves of these facilities. Thus, I have repeatedly since 1916 performed tests on needles used by the Nottingham embroidery industry. The manufacturers found that the British made needles were inferior in action to those formerly imported from Germany. On close examination I detected small differences in dimensions between the two makes; but these differences proved to be vital, for when, on repeated trial by the makers, they were eliminated the British product worked perfectly.

There are other well-known troubles in the local textile industries, notably in the non-standardisation of hosiery needles and in the uncertain length of yarn run off for a given weight. These are problems for which metrology can supply solutions.

It may be thought by some that the National Physical Laboratory is the proper place for all this testing, and that in undertaking it we should be usurping the functions of that Institution; but I would offer one or two remarks in opposition to this view: (1) The laboratory at Teddington will have enough to do with primary work and the more difficult tests, without handling the smaller work from every industrial centre in the country. (2) It is good for manufacturers to have a testing laboratory in their neighbourhood. Without such local facilities they will tend to get along, as heretofore, without metrology. (3) A metrology department can only keep in touch with industrial needs by taking in testing work; this vitalises it. (4) So far from robbing the National Physical Laboratory of testing work, provincial departments may well act as feeders to this national testing house; the more science you give the industries the more they will want De-centralisation appears to be the best way of spreading scientific methods throughout the country. The N.P.L. will always do the highest class of work and will remain the court of appeal.

I am not unaware of the difficulties attending metrological testing. The responsible head of the department must have a special training in the subject; and he would do well, whenever possible, to institute a system of cross-testing and checking, seeing that errors in metrological measuring and calculating are easily made and are often fatal.

Research.—This would take the form in the case of metrology of improving the existing methods of measuring and of introducing new methods as required. Thus, the methods of measuring screws, both external and internal, at the National Physical Laboratory, and the general screw-measuring machine which we have produced in Nottingham (*see* "Engineering," January 24, 1919), all in the last three years, show the kind of research that is wanted. These methods are adaptable, accurate and sufficiently speedy. Their value to the engineering industry should be incalculable. It will often happen that an industry in a particular district will present a problem peculiar to that industry for solution. This could be worked out by the local metrologists in close touch with the manufacturers.

I have tried to show in this brief sketch that the general introduction of metrology in the universities would materially assist both the teaching

and the research in physical sciences carried on within their walls, and would be valuable to the industries in the surrounding district. Pure science has been in a state of suspended animation for the last four years, while scientists in general have given themselves up to scientific war work; but, while research in pure science will now be restarted, it seems certain that industry will require, and will obtain, much more assistance from men of science than in pre-war times. The old attitude of aloofness between science and industry in this country is, one hopes, now to be finally abandoned.

Dr. P. E. Shaw said he was glad to hear Sir Richard Glazebrook express the view as to the desirability of disseminating metrology throughout the country. His own scheme as outlined in the Paper already in circulation on "The Need of Metrology in the Universities" might appear to have only an oblique bearing on the subject under discussion, but, in reality, he deemed it fundamental, as suggesting a practical way of bringing metrology and industry into close association.

The most striking result of the recent great development of metrology for war purposes was the hold this science has now acquired in the whole engineering industry. It is no longer merely the cult of a staff of experts in a Government bureau working in connection with a few specialist firms, but is now a widespread science, and has permeated to every workshop, large and small, in the country. Its recent achievements are no mere flash in the pan due to the war. These are presumably destined to be greatly exceeded as to scope and depth, both in engineering and in other industries.

In setting it up in a university, teaching would be undertaken to both day and evening students. Unfortunately for the teacher, there is at present no text-book, good or bad, on the subject, though there are some important monographs on individual points. The industrial class is virgin soil for the teacher. When one is assured by works managers that a large proportion of skilled mechanics do not know how to use even a micrometer, it is evident that our artisan class needs instruction in precision measuring. Besides artisans the advanced day students would benefit by an acquaintance with metrological method and appliances. The enthusiasm of the student so often damped by using rough laboratory equipment would be kindled afresh by handling the excellent metrological apparatus now obtainable. When the inexperienced undergraduate is using the crude appliances too often found in a teaching laboratory he may well be excused for doubting whether his feet are being set in the path of an *exact* science. Besides the teaching in a university department there would have to be some testing work undertaken. Without this kind of work the subject lacks vitality. Mere academic measuring would never have the interest appertaining to tests on actual products about to be used industrially. The provincial department would derive its standards from the N.P.L., and it would have to refer to this central bureau in much of the more difficult work for which the specialised department at Teddington is so eminently fitted. As an instance of useful testing work which could best be performed by a provincial testing house, Dr. Shaw quoted work on which he had been engaged on for years in connection with the textile industries, chiefly on needles. These and the various yarns proved to be

uncertain in dimensions and strength. The obvious remedy for all these vagaries was standardisation through the agency of metrology. As another sphere of activity the university departments would prove useful centres for research on the subject. The speaker had recently succeeded in producing a screw-measuring machine, which, when in a commercial form, he expected to provide a full solution of the old problem of measuring the diameters of internal screws.

Metrology has now attained the dignity of an independent subject. It was not a pure science, like physics, or an applied constructional science, like engineering. It should stand on its own feet, and be allowed full scope for development.

Mr. F. J. Dykes : I think I can claim to combine to a certain extent the academic and commercial side of gauges, and from the latter point of view I should like to say a few words. I have had the privilege of being connected during the past year with a firm which has been making gauges for sale for the last fifteen years or so ; the result of this experience is that I have come to believe that the first thing we want if we are to extend the use of gauges throughout the country is vigorous educational propaganda. In spite of munition work, we still have manufacturers who look upon gauges as cylindrical bodies made to standard sizes ; such gauges are rarely useful, and the one thing we must insist on, the one gospel we must preach, is the use of limit gauges ; these are the things which are wanted urgently. I am very glad to see that the Engineering Standards Committee is at last reconsidering its old decision to recommend the shaft basis for the system of limit gauges, and I think I need only say that the fact that the shaft basis system of the E.S.C. never took on, whereas the hole basis system is the only type which has reached any general use in this country, shows that the general trend of engineering opinion is in the direction of the hole basis from the commercial point of view of production. In connection with the Newall system, with which I was concerned, one great defect is that it does not go far enough. Even the A and B classes which engineers know, are too fine for many purposes, and we want one with a coarser tolerance, say, a C class, or even a D class of tolerance, which will take in every gràde of work, even that allowing "sloppy" fits, such as agricultural machinery or perambulator wheels, or anything you like. We want to educate the country to understand that limit systems can be used for any production whatsoever, and that has been the hardest job in the early pioneering work. Whatever England did in the past—I am very sorry we were backward in this country—the one bright spot in the early work was the fact that Japan welcomed the limit gauge system with eagerness, and I think that points a moral. We know that the class of labour one has in Japan is largely unskilled, and if we are going to extend production here it will be to the unskilled labour we shall have to turn ; we must never go back to the old system of one so-called skilled man, one job, one bit of steel. We must make it an operation production, and not a one-man job. Then, turning to the actual production of gauges, we want to make them cheaper, and one thing that can be done there is to educate the manufacturers so that they will not ask for special stuff. After the armistice inquiries came from all quarters, and, taking the screw gauges only, there were something like

two standard screw gauges wanted to 400 special threads. To give a concrete instance, motor-cycle manufacturers in very rare cases use standard threads, and we received inquiries for gauges for such hybrids as screws 18 mm. in diameter and 16 threads per inch, where $\frac{5}{8}$ in. B.S.F. is very close indeed. Why need they go outside the list of standard screws ? Besides that, they could get their gauges from stock, and every engineer knows what it means when something can be sent for from stock. Now, if I may appeal to our friends at the National Physical Laboratory, it would be to ask them to settle two classes of tolerances now for standard commercial gauges, and to announce that they are prepared to test gauges to those tolerances. People send inquiries for gauges, and ask at the same time, "Will they pass the N.P.L. test ?" We were in difficulty as to answering that question, as we did not know what "passing the N.P.L." meant ; also, if we could have two classes of tolerances, we should have the very good gauges we have at present, and another class, not quite so accurate, perhaps, which could be used, nevertheless, in cases where the buying of a very expensive outfit would not be justifiable ; also, provided the upper limits of these tolerances were the same as in the A class, the gauge could be relegated to the B class when worn, and we should double its life—a very important point when we come to consider the outlay on gauges. That would also get over another difficulty, I think, for possibly some of our present munition tolerances have been too close. I say this because I know what excellent work had been done before the war by gauges which would certainly not have passed the N.P.L. munition test, and I do not think I am giving away any trade secrets, but am merely stating a matter of common knowledge, when I say that a number of the fuses turned out during the war would not pass all the inspection gauges, so that it was necessary to resort to selective assembly in some cases ; still, the fact remains that, thanks to Sir Henry Fowler, they got the fuses out, and they certainly did their job. But if we are going to get the widespread adoption of the gauge system, I feel that we must cease to strive after ideal gauges, and have commercial gauges which will give commercial results. The ordinary manufacturer is shy of spending too much capital on his gauge equipment ; when he is educated it will be different. Lastly, I should like to touch on something which arises out of metrology, and to put in a word for the optical projector, for which we are indebted to Sir Richard Glazebrook's department, as an ordinary tool-room tool. In my own experience I find that when this was first put in the men were shy of it, and refused to use it ; gradually they began to use it, and now they refuse to work without it. So I hope that some manufacturer will give us a projector rather more robustly made than the present type, and one which the ordinary British workman can handle without the necessity of overhauling it once a week ; I want to see the projector used as a tool, and not as a scientific instrument.

Mr. M. F. Ryan, Director of Munition Gauges, Ministry of Munitions : My excuse for taking part in this discussion is due to the fact that for the past four years I have been very intimately connected with the application of metrology in the very difficult work of establishing a supply of gauges for dealing with the inspection of munitions. When this work

was taken in hand there were two great difficulties that hampered the supply of gauges for the innumerable types of munitions, such as mines, guns, aircraft engines, and every kind of store connected with the war. These two difficulties were due to the lack of experience of accurate methods of measurement, both of those who laid down the specifications for the gauges, and also of those who were making the gauges. With regard to the specifications, the difficulty, I think, arose from the following cause: A Government department responsible for the inspection of any stores was responsible also for the gauge designs. From the gauge drawings certain standard gauges were made, and then measured up in their laboratory and established as reference standards. Our experience was that the measuring instruments used for measuring those standards were by no means close enough to measure up within the tolerances. The result was that the standards established were false, and the drawings sent out to manufacturers to work to might be accurately worked to, but the product—the gauge produced—did not fit the reference standard. This hampered the production of gauges more than anything else at the beginning of the work, because it discouraged the manufacturer, and those who drew up the specifications had a false idea of the degree of accuracy that could be readily attained to. As soon as the National Physical Laboratory took up the work, this state of affairs was quickly exposed, and the result has been that the tolerances laid down on gauges now, as compared with the tolerances at the beginning of the war, are about three times wider. We want to have as wide a tolerance as possible on the gauge, so that the gauges can be manufactured commercially. (I may mention that the production of inspection gauges just before the armistice amounted to something over 10,000 a week.) The next point is lack of knowledge of metrology in the workshops. The education of the workshops was carried out by the National Physical Laboratory (Metrology Department), and I see that Mr. Sears and Mr. Dudding are here, and they will be able to give exact information as to how they get over these particular difficulties; it is due almost entirely to their work in teaching the various gauge-making contractors throughout the country how to measure and how to measure accurately that the rate of supply which I mentioned was secured. When gauge-makers were under the control, as it were, of a Government department, and under the supervision of the National Physical Laboratory, rapid strides were made, and the progress in metrology in gauge-making works and in the tool works of the manufacturers of aeroplane engines and other parts during the last two years is probably equal to ten or twenty years' progress under normal conditions. This is due, as I remarked, to the very excellent work done and the very great care taken to help the manufacturer by the N.P.L. It would be a very good thing to extend the idea, and I am heartily in favour of what Dr. Shaw recommends—namely, the setting up of metrology departments in the various universities. Each university can do the same for its own area as the N.P.L. has been doing for the whole country. The result will be more accurate work, the people in the works will be educated, and they will not be afraid of micrometers; micrometers, indeed, will probably be obsolete in fine work, they will have to work to minimeters. But in order really to make progress it will be essential that work shall be done to limit gauges. There are

signs that many manufacturers who during the war had been accustomed to work to limit gauges are going back to their old methods, and it is only by education and by educating in particular the young school of engineers that the old uneconomical methods of production can be finally abolished.

Mr. J. E. Sears, jun., Supt. of the Metrology Department, National Physical Laboratory: I want to make a few remarks from the point of view of the National Physical Laboratory, and particularly of my department, as to our work in the past, slightly, and more particularly as to our future programme. The war has given us a unique opportunity in connection with this matter of gauges. Our intimate association with the manufacturers of gauges and also, to a less extent, but still to a considerable extent, with the manufacturers of stores for which the gauges were intended, and also with Mr. Ryan's department, which has been the life and encouragement of the work on both sides, has enabled us to attain progress quite exceptional in the history of the subject, for ourselves, as well as for those whom we have been endeavouring to aid outside. It is now quite evident, I think, that this particular phase of munition work—limit-gauge work—is going to be a very important factor in all future engineering industry, and as Sir Richard Glazebrook said at the beginning, it is evident also that limit gauges, if they are to be applied as a national proposition as distinct from a local proposition—and by "local" I mean in the individual firm, and by "national" I mean securing that the products of one firm or one industry may be interchangeable with those of another—it is evident that all the controls of such gauges must be referred to a central body governing the fundamental standards of length. That body should be the National Physical Laboratory, and the first work of the National Physical Laboratory is the maintenance of the first order standards and the very highest class of work—investigation and research—in connection with them. Metrology, of course, covers a very much wider range than merely gauges, and we hope that our sphere of usefulness in connection with the industries will not be confined solely to the particular industry of mechanical engineering, but that we shall be of service in a similar way to other industries. I might mention the sort of subjects involved. Metrology is the science of pure measurement, and it has been limited hitherto to measurements of mass, length and time, and to simple derivatives of these, such as pressure, volume and mechanical velocity or density, and we have not had any *affaire* with such other subjects—except in so far as they are incidental to our work—as optics, thermometry, or electricity. Dr. Shaw's Paper suggests that these may be part of the sphere of metrology, but the scope of the subject is already sufficiently wide.

The point I want to make particularly is that we still need to keep in very close contact, as we have been able to do during the war, with the industries for which we are working. Unless that is done, any scientific department is apt to run off the lines a little, and to chase very interesting investigations which may, nevertheless, not belong to the particular phases of work which would be of most value; and we do want to maintain that intimate contact we are enjoying at the present moment. Metrology is rather a peculiar thing, because, although it is

a science in itself, as has been pointed out, it is unlike other sciences in certain respects. Metrology is not a science of great discoveries or inventions ; it is the science of doing with very great accuracy very simple things, and for that reason the main stimulus to progress is to be found in the interest which one gets out of seeing its application daily to routine work of industry. In the same way we want to keep routine work closely in touch with the developments of metrological science, in order to maintain the daily work of the industries up to the best standards which can at any given stage be attained. It is also necessary for the Laboratory to do a certain amount of routine work itself in this connection, partly because that is one excellent means of keeping the association between ourselves and the manufacturers, and partly because it is only by having the experience so gained that we can really get the stimulus to advancement which we require. I do not wish in saying that to contradict Dr. Shaw's arguments at all ; I think there is plenty of scope for the local centres as well as for the National Physical Laboratory, but it is important to get quite a considerable flow of test work through the Laboratory, as well as our scientfic work, in order to balance the various aspects of the case, and I hope that we shall get a good deal of routine testing work to do still, partly of a commercial kind, and partly, perhaps, official work for Government departments. It is necessary, in order to carry out that sort of work, to have a sufficient flow of material so as to organise it on a substantial basis. If you only have a few gauges coming to you for test, the trouble of getting out your standard gauges, tidying up your machines, and preparing to do the work, and then putting it all away again immediately afterwards, renders the whole operation inefficient. In order to attain efficiency you must have a reasonable demand for the class of work involved. The stimulus which the demands of the war gave to improvement of methods and greater speed and accuracy in testing has been invaluable, and we still want to have the same stimulus, although we do not want always to have the question of speed forced upon us to an extent which almost overwhelms every other consideration.

Then there is another point which I want to bring out. Accuracy of manufacture depends on accuracy of measurement, but in its turn accuracy of measurement depends on the accuracy of the measuring machines. We were, to start with, very much hampered by lack of manufacturing experience at the Laboratory. Eventually we got a quite small workshop, and that has done yeoman service in the development of the machines mentioned. The Ministry authorised the erection of a much larger workshop, which is now approaching completion. Accuracy of measurement frequently depends as much upon perfection in the manufacture of the apparatus as upon the design ; and the workshop associated with the Metrology Department ought to be such as to be capable of developing manufacture on lines of extremely refined accuracy—accuracy which is not required in ordinary commercial workshops. Such a workshop should be able, as progress is made, always to do something just a little better than is required in the best current commercial practice, and to devise improved methods where these are needed. I can refer, as an instance, to Johansson gauge work. We have depended very largely during the war on the use of Johansson gauges, and there came a time when it seemed doubtful whether the

supply would be maintained. We cast about to see whether something could not be done to make them, and I am very glad to say that in our little workshop my colleague, Mr. Brookes, succeeded in producing these things, and not only so, but we have reason to belive that in a larger workshop we could produce them much more cheaply than Johansson, and also much more accurately.

That is only one instance of the sort of thing which the metrological workshops might aim at. With it, and with the aid of the efficient designing department, with routine test work as a guide and stimulus, and with the upkeep of the fundamental standards and research connected therewith as the basis of the work, the department expects to be occupied in a balanced way, and to have information available which it is anticipated will enable us to give very valuable assistance and advice to engineers or to other industries requiring such assistance; and we hope to maintain our contact with them, and to encourage them to come to us for such advice as we can give.

It does not seem to me altogether desirable that the university or technical school should take charge of test work. The necessity for teaching metrology work in the university seems to me paramount, but the conditions of a university or technical school are hardly the best fitted for the routine test work, although I quite agree that local testing centres are desirable. The skill of the operator in measuring is largely a matter of experience, and you cannot ask students who can only devote a comparatively small portion of their training at the most to this sort of work to do such testing, nor, I think, is it quite suitable work for the teaching staff. The conclusion that I come to is that for the routine work you do want a special staff to be entirely devoted to it, and I think that the local testing office would be invaluable not only to the local industries, but also to the local university as an object lesson to students of the things that were being taught; but I doubt whether actual routine testing is quite the function of a university.

There is only one other point, referring to Mr. Dyke's remarks, the Laboratory *is* considering the question of tolerances, and hopes to do something in the matter quite soon.

Metrology at the University of Sheffield. By Prof. W. Ripper, *Ph.D.Eng. of the Department of Applied Science, University of Sheffield.*

COMMUNICATED BEFORE THE MEETING.

There is no doubt that industry has just emerged from war work with a new and additional experience provided by the production of repetition work for war purposes. Hitherto many of the older industries considered that the production of work in quantities necessarily implied a corresponding loss of quality. As a result of executing contracts for munitions, they have now found out by practical experience that it is possible to produce work in huge quantities, and by means of an efficient inspection to maintain accuracy and quality, which they previously could not obtain in the whole of their output.

The reason why the industries have been able to accomplish this when engaged on war products is because the Government has insisted upon an efficient system of inspection, which has had the effect of compelling the individual firms to adopt more scientific methods in the production of their goods.

One of the many lessons which has been learned is that of the practical application of metrology to industry, especially in the production of war material, and it is only by such application that it is possible to maintain an efficient standard of quality and at the same time obtain enormously increased quantitive output.

It is well known that the Universities and Technical Institutions throughout the country have greatly assisted the country by the use of their laboratories, equipment and staff in all departments of war work.

At the University of Sheffield the application of metrology to industry has been applied in metallurgy, engineering and glass. It would be as well to remind ourselves that Sheffield has for a very long time been doing this work for the steel industry. If we take as an example the work done in connection with engineering, the University has supplied 6,553 gauges, the accuracy of which has been of the highest grade. Latterly this work has developed, and we have been engaged in the production of hardened steel reference checks for the testing of inspection screw gauges.

With regard to the practical application of the equipment of the University which has been used for gauge purposes, we are desirous of

Fig. 1.—A typical British Tap purchased from a retail store.

assisting the local industry engaged in the engineers' small tool trade. Fortunately, this industry is already combined in an enthusiastic Association of all the firms engaged therein, and it is with this Association that our experience will prove extremely useful.

In the University a complete set of appliances for the checking of screw gauges has been installed. This is no longer required entirely for war work, and it might be very well employed as a local reference department for the checking of gauges, chasing tools, &c.

Figs. 1 and 2, which illustrate the form of thread of two taps compared with the standard form, show the improvement in the work which is possible.

The question of the best system of gauging taps and dies is, of course, still under discussion ; but in the meantime the set of gauges illustrated in Fig. 3 has been made as a trial, in the hope that it may help in the evolution of a system.

Fig. 1 shows a commercial tap which has been purchased locally, and illustrates very clearly the need of applying an efficient inspection system to the work. The defects in the form of thread are so obvious

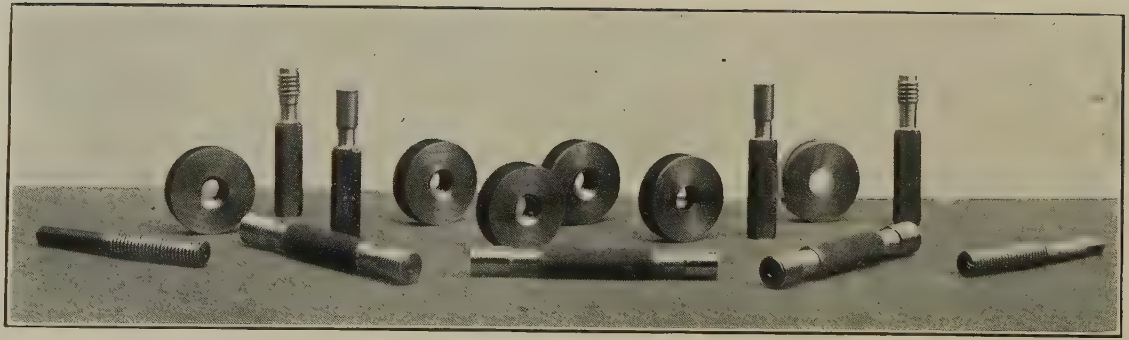


Fig. 3.—A set of Gauges and Checks for Inspection of Tap Diameters.

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that no comment is necessary. On the other hand one can buy taps from other firms, as an example in Fig. 2, where the form of thread is commercially good, and where a satisfactory inspection system is no doubt in operation. It would be as well to remember that in pre-war days the ordinary manufacturer of taps and dies did not concern himself at all with effective and core diameters, nor was he seriously troubled with errors in pitch and angle of thread ; but with the demand for producing tools that will cut satisfactory threads to pass inspection it is necessary that all these factors should be taken into consideration, and an efficient system of inspection should be used in order to ensure that all taps and dies when manufactured should be within certain agreed limits.

This modern tendency for more accurate work is now being catered for by many firms who are able to apply metrological inspection to their work, at the same time supplying the work at a competitive price.

Fig. 3 shows a set of gauges which can be used for checking both limits of each of the major effective and minor diameters of a tap, together with a set of checks for testing the accuracy of the gauges themselves. At the extreme ends of the illustration two taps are shown.

FIG. 2.—A SELECTED ACCURATE TAP.

This set was made to demonstrate to our local association a practical method by which all the diameters of the tap could be checked between the limits.

The illustration of this set of taps is not given as a standard set, but only as a set which was made to demonstrate to the Association one way in which all the diameters of a tap could be properly measured.

It does not necessarily follow that the application of an efficient inspection system in a works will retard output or increase the price. because the evidence which has been obtained during the war by numerous firms who have used inspection methods for the first time has been entirely in favour of increased production and lower prices.

The older industries of Sheffield have been awakened by a long spell of repetition work which the Government have required for war purposes, and they have seen the great advantage which has been obtained by standardisation.

The University inaugurated a course of lectures immediately the armistice was signed for the Cutlery, Edge Tool and File Trades. These lectures are given by a member of the engineering staff who had experience in the gauge work, and the lectures took the form of an engi-

neer's view of how to obtain efficient production in these various industries by an application of science to these industries.

In addition to the lectures, Technical Societies have been formed for the following: Cutlery, File, Edge Tool, Machine Knife and Saw trades, and most encouraging results have followed.

The societies are representative of manufacturers, staff and workmen, and they have all taken a very keen interest in the scientific side of their work. There is evidence of a demand for the practical application of science to their industries, and we are hopeful of research associations being formed in the near future in order to assist in the development of their industries along the most efficient lines.

What has been done in these older industries of Sheffield can be done in many other industries in the country, and one of the best methods is, no doubt, by the reading and discussion of Papers.

Mr. W. H. Bolton, Department of Applied Science, University of Sheffield: In considering this subject of metrology in industry I would ask you to look at it from two points of view—first, from the metrology point of view, how it can be applied to industry, and what are the practical steps to be taken; and, secondly, from the industry point of view, in which case we must look at industry as it exists to-day, and that not from the engineering side only, for we have had far too much engineering to-night and far too little of the general industry of the country. What steps could be taken to educate the industries so as to get them to appreciate the value of metrology? I would just ask your indulgence in considering some of the older industries. If, as one speaker has aptly pointed out, education is necessary amongst manufacturers—I presume he was referring to engineering manufacturers—in order that they might be got to appreciate metrology, what is the position when you come to compare some of the older industries where the work is still done by old rule-of-thumb methods? I would like to give you a comparison. The education of a student leaving a technical college might represent the engineering industry as compared with a child in the infants' department of an ordinary Council school representing the older industries. Not only is education necessary in these particular industries from the workman's point of view, but also from the principal's. Responsible men who are in the position of leading manufacturers do not understand the elements of metrology. They do not understand that efficiency in their business depends upon the successful application of metrology. During the war many lessons have been learned, and it has been one of the advantages of the war that it has led manufacturers to standardise and concentrate. I might mention that in the cutlery industry in Sheffield we had one firm manufacturing as many as 1,700 different patterns of pocket-knives. Now, gentlemen, we in Sheffield pride ourselves on our reputation for the production of cutlery. We claim that we produce still the finest cutlery in the world, but we have almost lost what is known as the "quantity trade," and this is due to the fact that we do not realise the value of the application of metrology to the industries. The cutlery works in Sheffield have never had educational opportunities, they have never known what the value of metrology is, and it has been to our competitors in other countries to take this advantage.

With regard to our own work in this direction, we have had some experience in the production of gauges, and we have got together a plant for inspection and for the production of this accurate work, which we do not intend to scrap. We think that the decentralisation of the means for making these accurate measurements is absolutely vital to the interests of the country. We think that the industries want vitalising by having many centres in which testing and checking can be carried out. We agree in believing that all these centres should be in close co-operation, and that their standards should be uniform, but I disagree with the last speaker in the application of testing; if this is necessary to the National Physical Laboratory in order to keep it in close touch with industry, it is more than necessary for the local Universities and Technical institutions. If we in Sheffield could put students on to testing work we should feel very proud, but if you are going to educate the best type of engineers in your technical institutions, then I submit you must have practical men to use the instruments and explain the instruments, and unless these men have some practical work to do, how are you going to get your demonstrations ? We have had far too much of the sort of teaching in our Technical institutions in which the results of the work are thrown under the bench. What better demonstration could you have for students than that of practical testing ? That applies generally to the education of engineering students. I should like to refer to one branch of engineering which we have in Sheffield—namely, the Engineers' Small Tools Manufacturers' Association. They have been working in the past, along with many other industries, on old-fashioned lines, and still to-day, I am sorry to say, they do not recognise the necessity for the successful use of gauges. They have not even been educated up to the point of being able to make their own gauges. But we have had a round-table discussion with this association, the result of which is that they have considered the problem, and have decided to work in co-operation with our local Department of Applied Science, and I believe the time will come when they will be successful, not only in adapting their methods of small tool production to metrological work, but also in producing for themselves the gauges they will need, making them to the fine limits which will be required.

With regard to the older industries, it is there, I think, where a good deal of help is necessary. Sheffield is not the only city where there are old-fashioned industries which require help. Such industries are scattered all over the country. But we recognise that the time has now come when these industries must be wakened up, when they must be brought up to date, and when they must apply metrology in their own particular crafts. We have tackled this subject by calling together the manufacturers, workmen and staff, and putting before them the advisability of a course of lectures from the point of view of engineering —that is to say, what would an engineer do with their factories and methods ? What would he do if he were in their industry ? Many engineers have considered the problem, and they have all repeated the same thing. They have said that it wants ending, that it cannot be mended. But we have formed in these industries technical societies for the discussion of difficulties, and although these societies have only been formed as recently as January they have done good work ; already one society has had five Papers, the fifth is down for to-night, and is being

given by a man who is practically self-educated and in a very small way. We have the promise of a Paper by a trade union official, who left school at the age of eleven, and altogether I am pleased to say that the workmen have taken a vital interest in this question, so that we hope by bringing the problems home to them and getting their difficulties from them to be able to educate them. But all this is theoretical work. However good the lectures may be, it is practical work which will move these men, and therefore we have taken our classes to the works. On Tuesday next we are taking a party of over 70 students—we call them students, although they are manufacturers, staff and workmen—to Manchester to see the organisation of one works there. By practical demonstration these people will be so educated as to see the possibilities of metrology. With regard to research, I believe that here is the key to the situation. We hope to be able to move the department of Scientific and Industrial Research to do something for Sheffield, and if that is possible to the extent that we require, then I believe that the Technical schools and Universities could follow on the same lines, and so bring home to the industries the necessity and value of metrology and the object lessons in its application. We believe in the engineering trade that efficiency depends entirely upon the applications of the scientific method, but I am sorry to say in many other industries they look upon this as theory. Therefore, we want education along these lines, and especially by discussions such as we have been having this evening, shall we arrive at that position when metrology will be of some practical use to industry.

Contribution by Bernard P. Dudding (Research Laboratories of the General Electric Co.): I think that the discussion has been too much confined to limit gauges—in fact, to a particular type of limit gauge, the screw gauge—and has not considered the large field covered by the science of metrology and its bearing on industry.

One might gather from the remarks of some previous speakers that all that is required to put English engineering in the forefront and keep it there is an unending supply of accurate gauges, and the general adoption of the limit gauge system of working.

Speaking in a general sense, before the war we were behind the principal American and Continental firms in the production of high-class machine tools, of measuring appliances and of scientific apparatus. At the present moment, in spite of some marked advances, the same criticism can be made, and as an instance, I wish to emphasise the impossibility of getting really good English-made tools to equip an up-to-date tool room.

The development of the production in England of accurate screw gauges to which considerable reference has been made to-night, has depended entirely on Pratt & Whitney measuring machines and on Brown & Sharp's micrometers—both imported articles, for which no really good English substitutes existed in 1916, and I doubt whether they do at the present moment.

I contend that what is wanted to advance the status of mechanical work in England is a thorough understanding of methods of making accurate measurements, and their application to all kinds of manufacture.

The limit gauge cannot replace this knowledge, and may only lead to the production of a large amount of second-rate articles, and with a tendency to improve the accuracy of the article.

Anyone who has had experience in branches of engineering requiring high accuracy cannot fail to have been impressed by the need for the knowledge of sound metrology in production of work of high precision.

In rougher types of work the errors can be seen without having recourse to measurement, and the corresponding improvements can be made, but as the workmanship improves the necessity for accurate measurement arises.

A manufacturer, say, of a machine tool, wishing to improve the accuracy of the machine, can only achieve this with certainty by being able to measure the existing errors or the errors of its products. Further, the experience he gains in making these measurements will be of tremendous value to him when considering new designs.

In workshops generally metrology is not used or understood, in fact, a man often uses a tool produced by a firm of reputation, and assumes it to be without error, or assumes it to produce work without error.

This is largely due to the fact that the subject of metrology has had practically no attention in technical schools and colleges, and very little standard literature can be found on the subject. It gives me great pleasure to note what has already been done to improve the education of English students in the science of metrology, by Prof. Shaw, at Nottingham, and by the Department of Applied Science, Sheffield.

I think that in most cases where metrology is introduced to help commercial engineering—*e.g.*, to introduce improvements in gear cutting, in machine tools, in scientific instruments, &c., a research will be necessary—firstly, to determine the methods of measurements to be used and possibly to develop new measuring devices; secondly, to attempt improvements in the actual production. The manner in which metrology can help the production of accurate mechanical work is well illustrated by the work that has been achieved in this country in connection with the manufacture of accurate screw gauges.

When the demand for large quantities of these gauges arose in 1915, the errors existing in the best make of English gauges were many times larger than the tolerance called for, and the average gauge made in the tool rooms of English firms had errors 20 to 30 times these tolerances. Although experience led to the tolerance being doubled, a lot of improvement was obviously needed to get quantities of gauges of the required accuracy. The errors arose principally from ignorance of errors in the tool shape and its setting and ignorance of the errors introduced by the latter.

The methods of measuring in use at the National Physical Laboratory in 1915 were not suited for application to the workshop, nor were they suited to deal with testing of large quantities of varying sizes. The staff of the Metrology Division at the National Physical Laboratory had first to investigate methods of measuring, having the above two objects in view. Machines were devised to give speed and accuracy in measuring the diameters, using needles and V-pieces, and a projection apparatus was devised to enable an image (50 times magnified) of the axial section of the screw gauge to be viewed and its shape contrasted against the standard thread form.

The diameter measuring machines were made by Taylor, Taylor & Hobson, and introduced to the gauge makers and their use explained.

Projection apparatuses were also erected in the various shops, and the workmen instructed how to use them.

The National Physical Laboratory staff also spent a considerable amount of time in the screw gauge maker's shops investigating sources of error.

The number of screw gauges tested at the National Physical Laboratory rose from 300 per week in 1915 to over 3,000 per week in late 1917. The percentage of gauges up to the standard asked for in 1917 was about 10 per cent. in 1915, but between 75 and 80 per cent. in 1917.

The English made screw gauges produced during the latter period were as good as any that could be obtained from the best makers on the Continent or in America ; in fact, with the exception of gauges made by Johansson, of Sweden, the best English screw gauges were better than those made elsewhere.

The production per person employed had also risen immensely, in spite of the heavy dilution of the skilled labour with unskilled. The unskilled workers were largely women.

In developing researches of any sort, difficulties will generally arise in getting the work from the laboratory into the shop. The first thing necessary is that the people doing the research shall have intimate contact with the man who is actually doing the work, and I would emphasise that it is very necessary to stimulate the interest of the workman.

In conclusion, I wish to draw attention to the method of projection we developed. It will lend itself to the improvement of all manner of work, and it should be widely used by precision workers. This is evidently being done in America, and the American technical press has recently had descriptions of apparatus which are only variations on the original machine which was made in England, and sent over when America first entered the war.

It would be a pity if an apparatus originated in England were to be used more widely abroad than at home.

Sir Richard Glazebrook, in replying on the discussion, said : I do not think that at this hour I have really anything to add. The discussion has been to me extremely interesting, and I am glad to find how much is being done all over the country in this direction. Mr. Bolton's address from Sheffield struck me as being particularly important, and it is very reassuring to realise that not only in the engineering industry, but in other forms of industry, the importance of metrology is being grasped, and something is being done to make what metrology can do for industry more widely and more generally known.

Letters were received from Sir Frank Heath, of the Scientific and Industrial Research Department ; and from Major P. A. MacMahon, Warden of Standards, expressing their regret at being prevented from attending the discussion.

Contributed after the Meeting by Mr. Sidney A. Horstmann, *of Bath.*

Metrology as applied to the measurements of screws is perhaps the most interesting branch of the science. The quantity production of

high precision screw gauges has shown the possibility of manufacturing screwed parts to a much finer degree of accuracy than has been possible heretofore.

Before the war little was known of the methods of measuring screws, and when the Ministry of Munitions called for quantities of screw gauges the only source of information available was the National Physical Laboratory, and it speaks volumes for this Institution that screws of an accuracy not dreamt of before the war can now be produced commercially by several firms and institutions in this country.

Measurements of length and diameter to a high degree of accuracy do not present much difficulty to the manufacturer with modern equipment, providing he has fundamental length bars or blocks for reference, but the measurement of the elements of the screw is an operation requiring far more knowledge, and the fact that a screw may measure correctly on all elements and yet be malformed still further complicates the subject.

From a gauge making point of view a screw does not present so many difficulties as it did, owing to assistance given to manufacturers by the National Physical Laboratory; but manufacturers of screw components are constantly in difficulties with their product, mainly due to the malformation of threads and pitch errors. The manufacturer may use gauges, often he does, but even if gauges are used and they fail to pass the work, the reason may not be obvious, and he has to resort to "trial and error" as a means of cure.

This all seems to point to the necessity of each manufacturer being provided with some form of projector, and sets of correct screw form, so that not only his gauges but his product as well may be reflected on the screen to a known magnification. The use of the projector is undoubtedly one of the most important advances made during the war in the examination of screws and other gauges, and its universal use by manufacturers would be of inestimable value to the industry generally. All manufacturers will not go to the expense of sets of screw gauges, especially where the article is not of standard size, and special gauges are required for checking, but with the use of a projector, preferably one that is able to check pitch errors as well, the operators are able to see the exact errors in their production, and it will be found that great interest is taken in the tool-room in producing forms of thread that will bear magnification, so that cutters, chasers, dies, &c., would all improve in form by having to be examined in this way.

Samples of work produced with die-heads, automatic and otherwise, can be readily examined, and providing the forms and pitch are reasonably right, outside diameters are quite sufficient for checking purposes. A good deal of education would be required in the various tool-rooms to help operators to correct for pitch errors in die heads, and should the product show considerable pitch error it would be necessary to examine the die-head chasers for pitch, and if these did not reveal the trouble the helix angle of the chaser would have to be checked, as this is a frequent source of trouble in die heads. This again could be done by means of a sine bar attached to the projector to which the chaser would be clamped, and which would be adjusted until a clear definition was produced on the screen of all flanks.

Measuring machines for diameter and pitch should be among the equipment of every modern tool-room, and this is generally acknowledged among manufacturers. The projector is looked upon as a luxury, but the variety of uses it can be put to makes it far more valuable than either of the foregoing, and its use would largely eliminate the multiplicity of screw gauges required especially where small quantities of articles with non-standard screwed portions are concerned.

The use of wax, graphite, sulphur and dental plaster for taking impressions makes it quite possible to examine internal threads quickly and easily.

Prof. J. B. Henderson, *of the Royal Naval College, Greenwich, contributed the following remarks, dated March* 29, 1919.

I regret that I had to leave the meeting last night, due to another engagement, before I was called upon to give my promised contribution to the discussion on the subject of metrology.

One very important point which was not mentioned while I was present is that of the influence of the accuracy of workmanship upon the factor of safety required when dealing with materials subject to repeated stress. No high degree of accuracy is required when dealing with ductile materials such as mild steel subjected to more or less constant stress, because the ductility of the material overcomes any defect in the fitting by local yielding. When, however, the material is subjected to alternating stress, such stress becomes a source of weakness. In order to illustrate this point I may mention the case of some test specimens which were sent by a Government department to the Engineering Laboratory at R.N. College, Greenwich, to be tested on the Haigh alternating stress machine, which gives an alternating pull and push test at a frequency of about 30 cycles per second. The specimens for this machine have screwed ends $\frac{1}{2}$ in. diameter, screwed with 20 threads per inch and the parallel middle portion of the specimen is $\frac{1}{4}$ in. diameter. All engineers will agree that the factor of safety for the screw threads in these specimens is ample, and no case of failure of a specimen breaking in the thread had been experienced until these particular specimens were received of a special bronze, of which three out of seven broke in the screw thread. Whether the failures were due to badly-fitted threads or due to some peculiar property in the bronze has not yet been elucidated.

With respect to the proposed educational propaganda, a first step, and a very important one, is the education of all engineering draughtsmen, and the man in the street in general upon the significance of figures. Now that it has become general practice to dimension drawings in decimals, it is quite common experience to find a dimension of, say, 1/16 in. entered in a drawing as 0·0625 in., although tolerance in workmanship required may even amount to 1/32 in. on that particular part. The figures 0·0625 ought to indicate that the tolerance is one or two significant figures in the last place of decimals.

Educational propaganda ought to begin at the very bottom in the elementary schools by teaching the children in simple multiplication to multiply by the most important figure first; to deal with the thousands before dealing with the units; contracted multiplication and division would then become quite natural.

The President, in closing the discussion, said: I have no special knowledge of the subject under discussion, but, as a layman, it seems to me that one or two points have emerged from the discussion with very great prominence. The first is, I think, that under the stress of the last five years' work, many manufacturers have learned to make use of limit gauges, and by use of them have turned out work which they would have said was quite impossible five years ago. Another point is that there is a tendency at present for them to fall back into the old ways, but if we allow that tendency to go on we lose something for which we have fought vigorously, and which we ought, if possible, to retain. It is in this connection that the suggestion of Mr. Dykes seems to me worthy of very careful consideration, namely, that for those manufacturers who hold that in their business the highest type of gauge with its small tolerance is not necessary, there ought to be provided an inferior class of gauge with a wider tolerance. In the course of time these manufacturers might find by experience that even the highest type of gauge would be useful to them. Another point that seems to have been brought out by the discussion is the advisability of having distributed over the country sub-stations, at the universities, or elsewhere, where the accurate measurement of gauges could be carried out. These stations would be run either by the National Physical Laboratory, or, if independently, in very close connection with that institution. Many speakers have emphasised the necessity of keeping the measurement in close contact with the manufacture of gauges, and the further necessity of keeping research in contact with measurement; so that the manufacture, measurement and research on gauges should all be in close contact with each other.

Throughout the discussion I have been very much struck by the familiar and almost affectionate way in which people have spoken of the National Physical Laboratory as the "N.P.L.," and I am sure Sir Richard Glazebrook feels the compliment paid to the institution in the use of that abbreviation.

Lastly, I should like to thank Sir Richard Glazebrook and all the speakers in the discussion; also Dr. H. S. Allen, our secretary, and Dr. P. E. Shaw, who have taken so much trouble in organising it.

An exhibit of optical scales and graticules by Mr. J. Rheinberg was on view, and in closing the meeting the President called attention to it.

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